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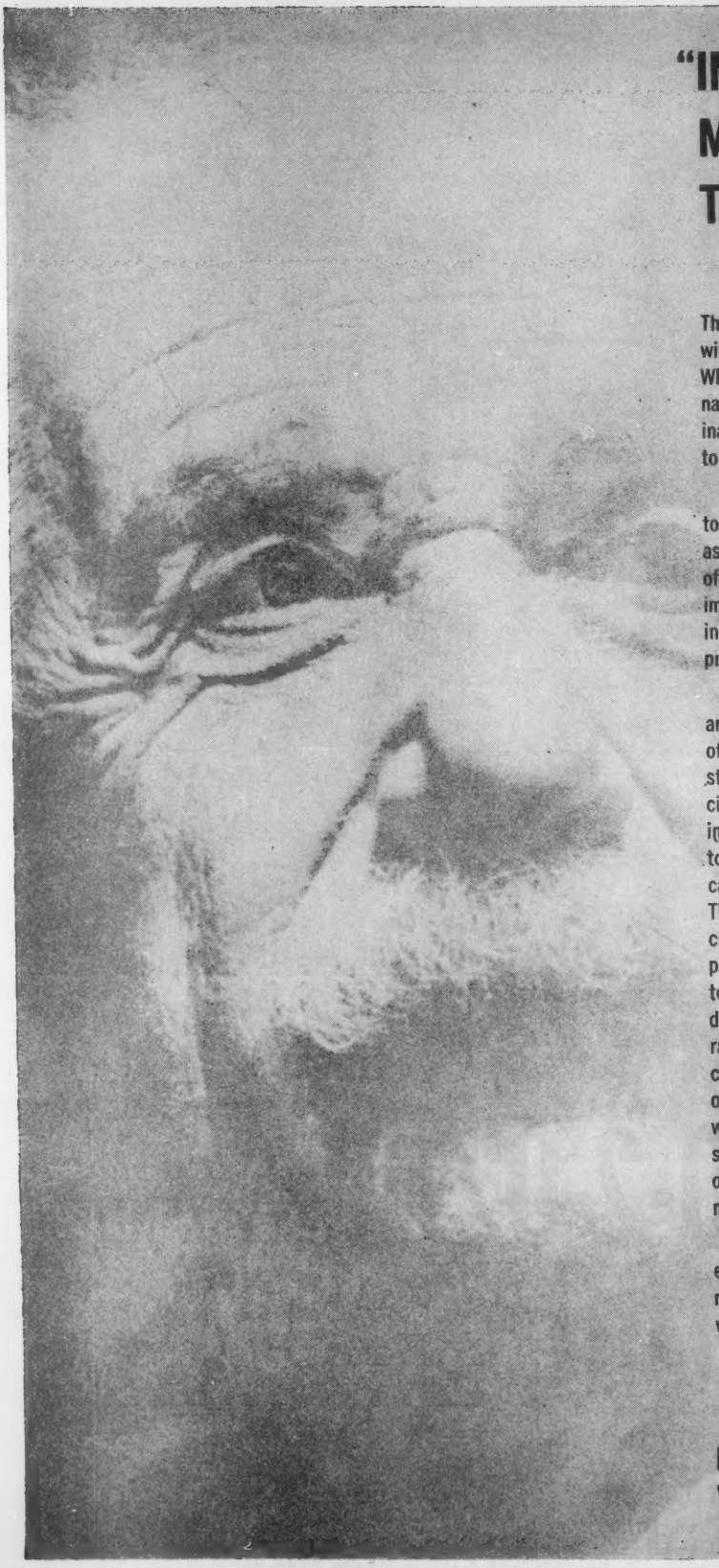

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Cover

Human figures engraved in Addaura cave on Monte Pellegrino at Palermo, Sicily, during Paleolithic age. The two central figures, with sharply bent knees and with feet apparently tied to the neck by a rope, are surrounded by other figures, some masked and some having thick hair. These realistic figures are substantially different from the exaggerated human figures of most Franco-Cantabrian art. The figure at the center measures 24 centimeters. [From *Palaeolithic Art*, reviewed on page 748]

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Reading the Fine Print

In Geneva next week a number of Americans, some of whom have made the trip before, will sit down with their British and Soviet counterparts to attempt once again to write a treaty banning the testing of nuclear weapons. Among other matters still to be settled are key scientific disputes concerning the design of an inspection system that will deter cheating without permitting espionage. Not only must solutions be found to these scientific matters, but diplomats and policy makers—and the educated public, too—must understand these solutions. If some of the technical disputes appear minor, tiresome, and overly subtle, just how they are settled can have enormous consequences for good or ill if an inspection system is ever really established.

By way of illustrating an apparently minor point with major consequences, consider the American and Soviet proposals on the conditions under which, in the monitoring of underground explosions, an area becomes eligible for on-site inspection.

The American proposal sets a limit (actually two limits, depending on the kind of data available) on the size of an area that may be inspected. It then requires that once a seismic event is judged suspicious, that is, as possibly not a natural earthquake, the team selects the area that has the highest likelihood of containing the event. The meaning of this position may be clarified by a hypothetical example in which a house represents a nation and one room represents the size of the area that may be inspected. Imagine that a suspicious event has occurred in the house; that the inspection team evaluates for each room the likelihood that the event occurred there; and that the evidence is such that these likelihoods range from 65 percent for the kitchen to 3 percent for the nursery. Under these conditions, the team would inspect the kitchen.

In the Soviet proposal, there is first the complication that the position is not completely clear to the American negotiators, so that the problem is not only one of comparing proposals but of determining the meaning of words. The Soviet proposal requires, before inspection may take place, that the suspicious event be "localized" within an area of a certain size (the size is not larger than that proposed by the Americans for inspection). In this proposal everything depends on the meaning of "localized." If, for example, the word means that the whereabouts of the event must be known with a likelihood of 95 percent, then the Soviet procedure would, in general, make inspection more difficult than the American procedure. In the particular circumstances we have imagined, it would make inspection impossible. In our hypothetical example, the team would not be permitted to inspect the kitchen because the likelihood that the event occurred there is only 65 percent, not 95 percent.

Although the Soviet proposal does not specifically say that the area that may be inspected is the same size as the area within which the event must be localized, the hypothetical example assumes this to be the case. If this is not the case, things could be even worse. Suppose, under other circumstances, that the likelihood that the event occurred in the kitchen actually is 95 percent. Inspection still would not be very effective if it were limited to an area the size of the sink.

We are not, of course, going to get perfect security, but this does not mean that we should settle for anything less than a sound treaty. An unsound treaty would be worse than the present *de facto* ban, which, at least, has the merit of not pretending to be based on an inspection system. We should all hope fervently that the coming negotiations rapidly produce a treaty, but our hope must not allow us to grow impatient over what seem to be minor technical matters.—J.T.

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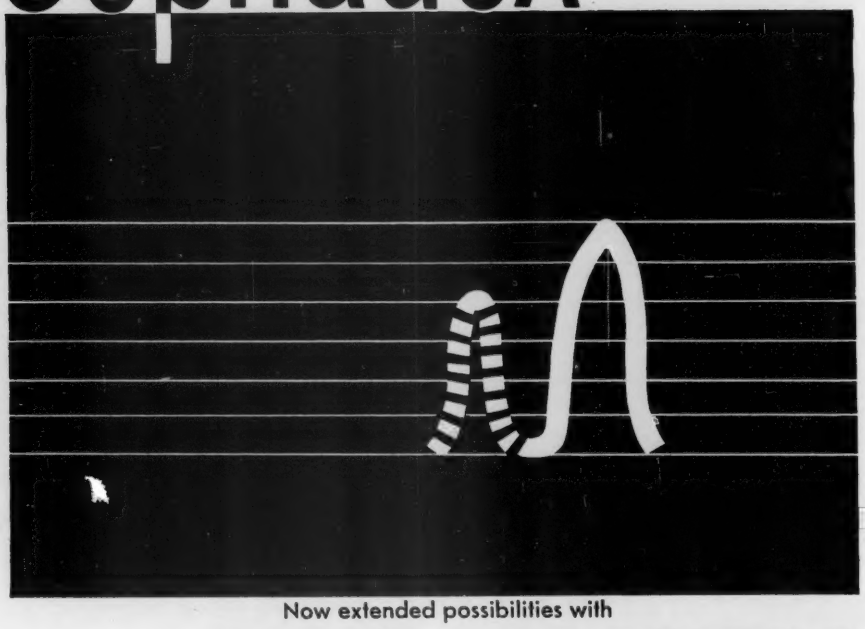

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17 March 1961, Volume 133, Number 3455 SCIENCE

Frozen Mammoths and Modern Geology

The death of the giants can be explained as a hazard of tundra life, without evoking catastrophic events.

William R. Farrand

Frozen woolly mammoths have perplexed both scientists and laymen during the several centuries since the first direct description of a frozen mammoth was recorded, in 1692. In the words of Digby (*1*), a well-known mammoth hunter, "the gods must have enjoyed many a hearty laugh over humanity's attempt to account for the remains of mammoths." One of the biggest obstacles to complete interpretation of the frozen mammoths was, and to a lesser degree is still, the lack of detailed knowledge of the distribution, geologic context, and age of the beasts. Tolmachoff (*2*) wrote a very complete summary of the information available in 1929, but no comprehensive paper has appeared since that time, although many more geological data are now at hand.

In contrast to scientific efforts, a number of popular and quasi-scientific articles (*3*, *4*) have appeared in recent years, in which fragmentary knowledge, folk tales, and science fiction are combined under the guise of verity—much to the chagrin of scientists and the confusion of the public. The most recent of such articles is that of Sanderson (*3*), who comes to the conclusion that the "frozen giants" must have become deep-frozen within only a few hours' time. Such a thesis, however, consistently disregards the actual observations of scientists and explorers (discussed below). Adding insult to injury, Sanderson proceeds to fashion a fantastic climatic catastrophe to explain his conclusions.

Information from diverse, mostly European, sources (*5*) is summarized in this article to bring the subject of frozen mammoths up to date and at the same time to supply to scientists in general the information with which to refute the current quasi-scientific theories. Although the general ecology and range of the woolly mammoths is included, this article centers on the frozen representatives of the species and the special problems they present. All other species of mammoths, such as the Columbian, Imperial, and Jefferson mammoths, are entirely excluded from this discussion.

The subject of extinction is not discussed here because it is not a problem peculiar to frozen mammoths but one that concerns other species of mammoths and many other large Pleistocene mammals as well. Let me say only that climate apparently did not play a direct role in the demise of the Siberian mammoths. Woolly mammoths were well adapted to extreme cold and to tundra vegetation—conditions which still characterize the area where frozen cadavers have been found. Furthermore, woolly mammoths lived in pre-Wisconsin and late-Wisconsin time, and this shows their ability to survive a glacial onslaught.

The author is a research associate in Pleistocene geology at Lamont Geological Observatory, Palisades, N.Y., and a member of the geology department of Columbia University, New York.

Description and Ecology

The taxonomic position of the woolly mammoth is rather well defined (*6*), although some controversy surrounds its generic name. *Elephas primigenius* Blumenbach 1799 has withstood many competitors throughout the years; it is found in much of the older literature and is still used in Europe. Currently, however, *Mammuthus primigenius* (Blumenbach) (*7*, p. 415; *8*) is used in North America and, accordingly, is used in this article, without any attempt to resolve the difference. However, histological examination of frozen mammoth bone from Alaska favors retention of the genus name *Elephas* (*9*).

Mammuthus primigenius (Blumenbach) was an imposing creature (Fig. 1), standing as tall as modern elephants but with slightly different proportions (*10*, p. 806; *11*, p. 129). The Siberian mammoths—2.8 meters tall at the shoulders—were somewhat smaller than the average European woolly mammoths (3.2 to 3.9 meters tall); modern Indian elephants are 2.7 to 3.2 meters tall, and African elephants average 3.4 meters. The woolly mammoth differed from modern elephants in the following important features: (i) the mammoth's head had a conspicuous topknot formed by large sinuses (*7*, Fig. 311) and possibly lumps of excess fat (it is well shown in prehistoric cave drawings in western Europe); (ii) the mammoth had only four skeletal toes as compared to pentadactylism in other elephants; (iii) its body was covered, or nearly covered, with long coarse hair and thick underfur; under its epidermis, which was identical with that of a modern elephant, was a layer of fat up to 9 centimeters thick; (iv) the mammoth's tusks were larger and more

curved than those of modern elephants, although not as curved as they are often depicted; Digby (*1*, p. 171) found in a collection of more than 1000 tusks that "not one tusk in ten forms a third of a circle, not one in twenty even a semi-circle"; and (iv) the slope of the back from shoulder to hip was greater in the mammoth than in modern elephants, and this was especially pronounced in the Siberian population.

The evolutionary development of Pleistocene elephants is rather well known; it is based mainly on dental characters (decrease in the size of dental plates and the thickness of enamel layers and increase in the height of teeth). The main line of mammoth development leads from *Mammuthus* (*Elephas*) *planifrons* and *M.* (*Elephas, Archidiskodon*) *meridionalis*, which were warm-latitude forest dwellers of early Pleistocene time, through *M.* (*Elephas, Archidiskodon*) *trogontherii*, a cool steppe dweller of the middle Pleistocene (Günz-Mindel interglacial into Riss glacial), into *M.* (*Elephas*) *primigenius*, a cold steppe and tundra dweller of later Pleistocene times [late Mindel(?) into late Würm] (*12*, pp. 457, 470; *13*).

The straight-tusked elephants (*Loxodonta*) were evolving during this same time in more southerly regions of Europe and Africa. Serological tests of the Berezovka mammoth (*10*, p. 794) and histological examination of Alaskan mammoth bone (*9*) indicate close relationship between the woolly mammoths and modern *Elephas* (Indian elephants). *Mammuthus* (*Elephas*) *primigenius* appears to have been a species adapted for extreme cold and tundra conditions, as shown by its smaller size and broad four-toed feet for marshy terrain and by a further decrease in the size of dental plates and the thickness of enamel layers. It seems, furthermore, to represent a dead-end evolutionary development.

The habitat of the woolly mammoth is indicated clearly by it physical appearance and food habits, as determined from the frozen carcasses and associated fossils. Long hair, thick wool, and a heavy layer of fat definitely indicate a cold climate. Stomach contents (*1*, *2*, *6*, *14*) reveal an abundance of grasses, sedges, and other boreal meadow and tundra plants, along with a few twigs, cones, and pollen of high-boreal and tundra trees. In general, this floral assemblage is "richer . . . , somewhat warmer and probably also moister" than the present flora of the tundra in which frozen mammoth carcasses are now found (*14*). Quackenbush (*15*) found "large trees" associated with fossil mammoth in a now-treeless part of Alaska and also came to the conclusion that the climate was somewhat milder when the mammoths lived. The flora of deposits enclosing frozen mammoth carcasses is similar to that of the stomach contents (Table 1). Furthermore, the healthy and robust condition of the frozen cadavers (*2*, p. 49) indicates that the mammoths fared well on such a diet.

The fauna of which the woolly mammoth formed a part was composed mainly of boreal and arctic steppe- and tundra-dwelling animals, although a few problematical warmer-latitude types

Fig. 1. "Stuffed mammoth found in the permafrost zone." This is the Berezovka mammoth displayed in the Zoological Museum, Leningrad, in the position in which it was discovered. The skin of the face and trunk were missing. [Zoological Museum, Leningrad, courtesy of J. H. Zumberge]

appear in some European deposits. The more complex character of this fauna, as compared to the flora, is easily understood in terms of (i) the migratory nature of the large mammals involved (*16*) and (ii) the fact that most of the faunal assemblages were found in western Europe, which in glacial times was a transition area between glacial and tundra conditions on the one hand and steppe and boreal forest on the other. Soergel strongly emphasizes the effect of seasonal migrations on the faunal assemblages of central Europe, and he points out (*17*) that woolly mammoths occur only in glacial or transitional (glacial/interglacial) faunas and not in *high*-interglacial assemblages.

Distribution

Whereas *Mammuthus primigenius* was widely distributed throughout most of northern Eurasia and northern North America (*6*, *10*, *18*), *frozen* remains of woolly mammoths have been found only north of latitude 60°N (mostly north of the Arctic Circle) and distributed around the Arctic Ocean, from the Yenisei River in Siberia to the interior of Alaska (*2; 10; 15; 19; 20*, p. 259). Such a distribution shows the relative abundance of the woolly mammoth in certain parts of the high latitudes and coincides with the present-day extent of frozen ground. Even *bones* of the woolly mammoth are rare in Scandinavia, and they are lacking entirely in most of the Canadian archipelago. Other than two very fragmentary carcasses from Alaska, all of the frozen cadavers have come from northern Siberia (Fig. 2). There have been at least 39 discoveries of frozen mammoth remains, with some soft parts preserved, but only four of these were nearly complete: Adam's mammoth from the Lena delta (recovered in 1806), Herz's mammoth from the Berezovka River (1899), Stenbock-Fermor's mammoth from Great Lyakhov Island (1906), and Vollosovich's mammoth from the Sanga-Yurakh River (1907).

The woolly mammoth that has been most intensively studied is that from the Berezovka River. The woolly mammoth most recently discovered, also well studied although by no means a complete carcass, was unearthed on the Mamontova River in the Taimyr Peninsula in 1948 (*20*, p. 259; *21*). On the other hand, fossil tusks of woolly mammoth are very abundant and have been collected by ivory hunters for centuries. Digby (*1*, p. 169) describes a single cache of more than 1000 tusks which he examined in Yakutsk, and Flint (*12*, p. 470) mentions some 50,000 tusks from Siberia alone. The obvious conclusion is that the frozen mammoths were members of a populous race located in Siberia (and elsewhere) and not occasional strays who happened to migrate beyond their normal range. And, contrary to some popular accounts, the figures cited above do not support the conclusion (*3*, p. 82) that "absolutely countless numbers" of woolly mammoths were frozen and that "many of these animals were perfectly fresh, whole, and undamaged. . . ."

The types of deposits that enclose frozen remains of woolly mammoths help us to reconstruct their habitat, but unfortunately, complete descriptions are not available for some of the early discoveries. "Mammoth-bearing drift" is described by Tolmachoff (*2*, p. 51) as usually constituting the locally high portions of the tundra and lying above sediments of the "last Arctic transgression" [the "Boreal transgression" of

Table 1. Plants found in the stomach of the Berezovka mammoth (*2*, *6*, *14*) and in deposits enclosing the Mamontova mammoth (*21*).

Plant	Berezovka	Mamontova
Trees and shrubs		
Abies (*sibirica*?)	+	
Alnus hirsuta	+	
Betula alba	+	
B. nana	+	+
Betula sp.	+	
Cassiope tetragona		+
Larix (*sibirica*?)	+	
Picea (*obovata*?)	+	
Pinus pumila		+
P. sibirica	+	+
Salix polaris	+	+
Salix sp.	+	
Vaccinium vitis idaea	+	
Herbs, grasses, and mosses		
Caryophyllaceae		
Arenaria Minuartia cf. *arctica*		+
Cerastium sp.	+	
Dianthus sp.	+	
Melandrium sp.	+	
Sagina nivans		+
S. (*nodosa*?)	+	
Chenopodiaceae		
Atriplex (*patulum*?)	+	
Compositae		
Artemisia borealis		+
A. dracunculus	+	
A. sacrorum	+	
A. vulgaris	+	
Aster sp.	+	
Erigeron cf. *criocephalus*		+
Gnaphalium uliginosum	+	
Lactuca [*Mulgedium*] *sibiricum*	+	
Senecio cf. *resedifolius*		+
Tanacetum vulgare	+	
Sp. indeterminate	+	
Cruciferae		
Draba sp.		+
Sp. indeterminate	+	
Cyperaceae		
Carex glareosa	+	
C. hyperborea		+
C. incurva	+	
C. lagopina [*lachenalii*?]	+	
C. cf. *maximum*		+
Eriophorum angustifolium		+
Sp. indeterminate (*N*-2)	+	
Gentianaceae		
Gentiana sp.	+	
Gramineae		
Agropyron cristatum	+	
Agrostis borealis	+	
Alopecurus alpinus	+	
Beckmannia cruciformis	+	
Bromus sibiricus	+	
Herbs, grasses, and mosses (cont.)		
Gramineae (Cont.)		
Elymus sp.	+	
Hordeum jubatum	+	
H. violaceum	+	
Phragmites communis	+	
Puccinellia [*Atropis*?] *distans*	+	
Sp. indeterminate (*N*-8)	+	
Labiatae		
Thymus serpyllum	+	
Leguminosae		
Caragana jubata	+	
Oxytropis campestris	+	
O. sordida	+	
Sp. indeterminate		+
Papaveraceae		
Papaver alpinum	+	
P. lapponicum		+
P. cf. *radicatum*		+
Plantaginaceae		
Plantago media	+	
Polemoniaceae		
Polemonium cf. *boreale*		+
Polygonaceae		
Oxyria digyna?	+	
Polygonum cf. *viviparum*		+
Rumex acetosella	+	
Primulaceae		
Androsace sp.		+
Ranunculaceae		
Caltha palustris	+	
Ranunculus acris	+	
R. Gmelini		+
R. pygmaeus		+
Thalictrum sp.		+
Rosaceae		
Potentilla sp.	+	
Rosa sp.	+	
Sanguisorba officinalis	+	
Saxifragaceae		
Saxifraga cf. *hirculus*		+
Scrophulariaceae		
Pedicularis cf. *sudetica*		+
Umbelliferae		
Aegopodium podagraria?	+	
Angelica (*decurrens*?)	+	
Lycopodiaceae		
Sp. indeterminate		+
Polypodiaceae		
Dryopteris sp.		+
Sp. indeterminate (*N*-2)	+	
Bryophytes		
Aulacomnium turgidum	+	
Cladonia rangiferina	+	
Drepanocladus [*Hypnum*] *fluitans*	+	
Sphagnum sp.		+

current Russian (*22*) terminology]. Marine fossils have never been discovered in deposits containing frozen mammoths (*2*). The Mamontova specimen (*20*, p. 259) was found in floodplain deposits postdating the Boreal transgression, and Digby (*1*, p. 55) said that "practically all cold-storage mammoths and woolly rhinos are found on the sides of cliffs sloping down to rivers —a lake in one or two cases." The frozen mammoth found in a bluff facing Eschscholtz Bay, Alaska, was buried in floodplain deposits, which also included a beaver dam (*15*).

Geological Age

Much discussion has centered around the age of frozen mammoths, but several lines of evidence now point toward a solution of this question. To begin with broad categories, we know that woolly mammoths do not exist at the present and that they originated no earlier than the Mindel (second) and probably as late as the Riss (third) glacial stage; therefore, the species occurred in the last half of the Pleistocene epoch.

Flint (*12*) judged the fauna of the Alaskan frozen muck, which includes woolly mammoth, to be an interglacial fauna, on the basis of some warmer-latitude species which occur there and a series of infinite radiocarbon dates (the oldest being "greater than 30,000 years"). But some finite dates have also come from the Alaskan muck: *Bison crassicornis* horn sheaths (M-38) were dated 16,000 ± 2000 years before the present (*23*), and skin and flesh of a baby elephant, possibly a woolly mammoth (L-601), were recently dated by the Lamont radiocarbon laboratory at 21,300 ± 1300 years (*24*). These dates show the complex nature of the muck (silt) deposits and point to the possibility that several, temporally separated faunas may be mixed together.

The area in northern Siberia in which frozen woolly mammoths are found can be subdivided with respect to the extent of Pleistocene glaciation, as defined by recent Russian work (*22*), and thereby can aid in dating some of the carcasses (Fig. 2). Almost all of northern Siberia north of the Arctic Circle and west of the Lena River was covered by glacier ice of the Last glaciation, the Zyryansky stage in Russian terminology (Fig. 3). East of the Lena only the uplands were glaciated, the arctic coastal plain in that area being left free of ice (*25*). In the latter area mammoth cadavers are found outside the area of glaciation, but they overlie marine sediments of the Boreal transgression. In the former

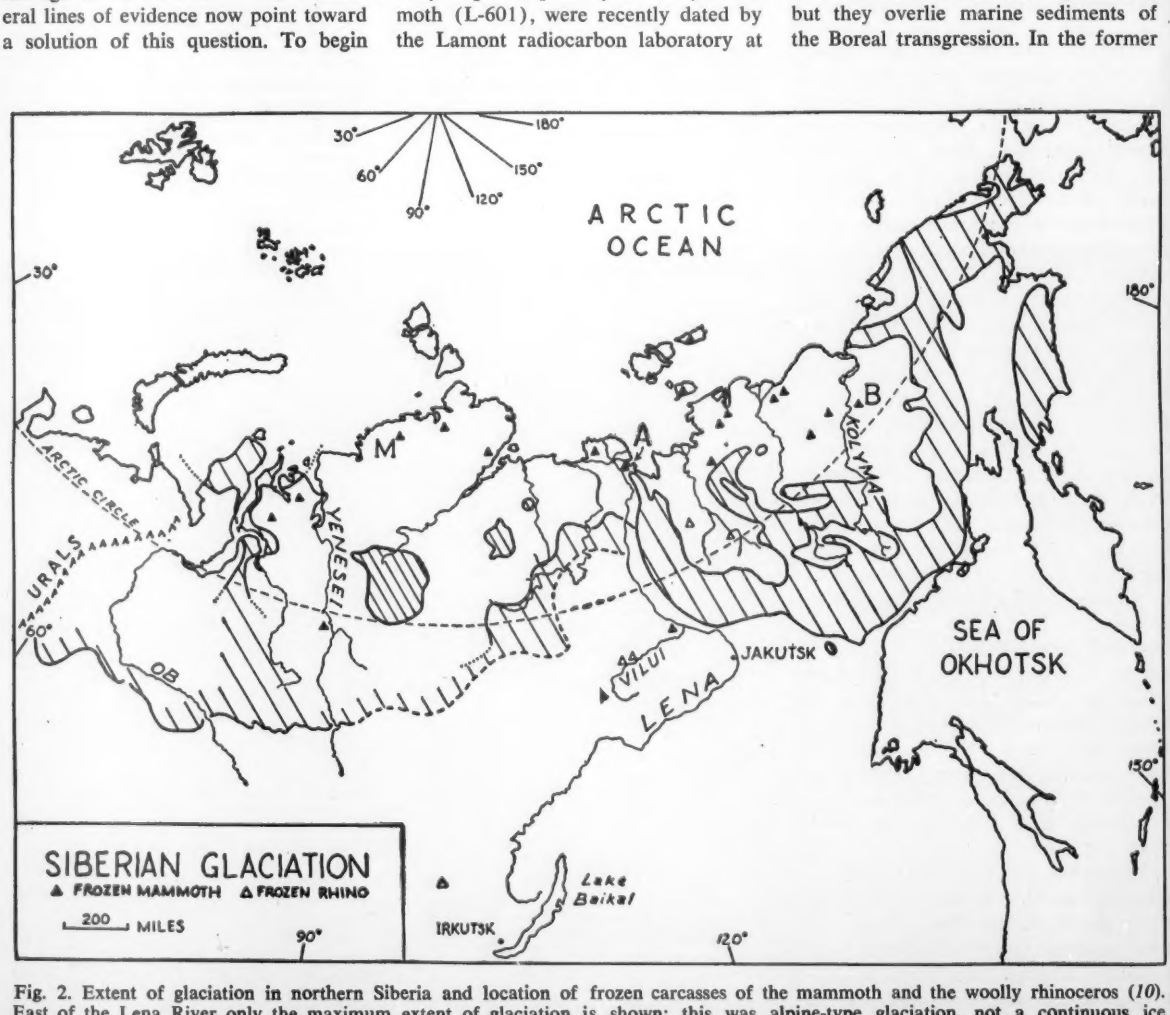

Fig. 2. Extent of glaciation in northern Siberia and location of frozen carcasses of the mammoth and the woolly rhinoceros (*10*). East of the Lena River only the maximum extent of glaciation is shown; this was alpine-type glaciation, not a continuous ice sheet. West of the Lena the maximum glaciation is shown by broad hatching; the Zyryansky stage (not hatched) and the Sartansky stage (closer hatching) are shown in so far as they are known. (*A*) The Adam's mammoth (dated > 30,000 years); (*B*) Berezovka mammoth; (*M*) the Mamontova mammoth. The extent of glaciation of the arctic islands is not shown.

area—that is, west of the Lena—mammoths are found above deposits of the Last glaciation which, in turn, overlie sediments of the Boreal transgression.

Russian geologists assign most of the mammoth remains to the Karginsky warm period, an interstadial which followed the maximum extent of the Zyryansky glaciation but preceded the final (Sartansky) expansion of now extinct glaciers of the northern Siberian highlands. It has been suggested (*22*) that this final expansion was equivalent to the Salpausselka moraines of northern Europe (Valders substage in North America), which formed about 10,500 years ago. Therefore, the mammoths *west of the Lena River* apparently lived in a relatively warm period prior to the close of the Last glaciation, probably the Allerød or Two Creeks interstadial, of Europe and North America, respectively—a period in which the climate was less warm than at present.

On the other hand, the flora associated with the Berezovka and Mamontova mammoths (Table 1) indicates a climate slightly warmer than the present, and the Lena delta mammoth (Y-633) was dated by radiocarbon as more than 30,000 years old (*26*). Both of these facts point to an interglacial age *prior* to the Last glaciation, a conclusion which is compatible with the occurrence of all mammoths *east* of the Lena River, including the Lena delta, which appears not to have been glaciated. The Berezovka and Lena-delta mammoths could, therefore, be remnants of the final part of the Last interglacial period.

But the Mamontova mammoth from the Taimyr Peninsula presents a problem: although the associated flora is a warmer-latitude flora than that found at present, the mammoth lay in an area covered by Zyryansky glaciers and apparently must postdate the maximum of the Last glaciation. The only period since the maximum of the Last glaciation in which the climate was warmer than at present was the postglacial Hypsithermal interval (*27*). If Popov (*20*, p. 274) is correct in his determination that this mammoth lived at the time of the second terrace above the present floodplain of the Mamontova River, then it probably predates the Hypsithermal interval. If is conceivable but unlikely that two periods of downcutting have occurred there in post-Hypsithermal time (*25*). Therefore, an apparent paradox remains—that the climate in northern Siberia was warmer than at present at some period in late glacial time when climates elsewhere on the earth were cooler than at present.

It is also argued (*29*) that some of the woolly mammoths lived in northern Siberia during the postglacial Hypsithermal interval (7500 to 4000 years ago), but the data on which such a conclusion is based are also compatible with late Last-interglacial time or an interstadial of the Last glaciation. Furthermore, although woolly mammoths are prominent in prehistoric cave art in late Last-glacial time, they are unknown in postglacial deposits in the much-studied areas of Europe and North America. Also, Griffin (*30*) reports that mammoths were hunted by Siberian Advanced Paleolithic people in the Lake Baikal area as late as 12,000 to 9000 years ago, during the waning stages of the Last glaciation, but that no mammoths of a more recent date are found in these sites.

In summary, Siberian frozen woolly mammoths are found (i) in deposits related in time to the Last glaciation, most of them dating from a major interstadial prior to 10,500 years ago, and (ii) in deposits apparently of late Last-interglacial age (postmaximum Boreal transgression). This time range agrees with that of the distribution of *Mammuthus primigenius* throughout Europe and North America (including Alaska), where it is found from late (?) Last-interglacial through late Last-glacial times.

Death and Preservation

All the evidence now at hand supports the conclusions of previous workers that no catastrophic event was responsible for the death and preservation of the frozen woolly mammoths. The cadavers are unusual only in that they have been preserved by freezing; the demise of the animals, however, accords with uniformitarian concepts. The ratio of frozen specimens (around 39) to the probable total population (more than 50,000) is of the order of magnitude expected among terrestrial mammals on the basis of chance burial. Furthermore, the occurrence of nearly whole carcasses is extremely rare (only four have been found), in spite of the numerous expeditions for fossil ivory and other exploration in northern Siberia.

There is no direct evidence that any woolly mammoth froze to death. In fact, the healthy, robust condition of the cadavers and their full stomachs argue against death by *slow* freezing. On the other hand, the large size of their warm-blooded bodies is not compatible with *sudden* freezing. In addition, all the frozen specimens were rotten and, in most cases, had been somewhat mutilated by predators prior to freezing. This is attested to by many first-hand accounts (*2*, p. 60; *15*; *20*, p. 274; *31*). Although some of the flesh recovered from the cadavers was "fibrous and marbled with fat" and looked

	SIBERIA	NORTH EUROPE	ALPS	NORTH AMERICA
RECENT	POST-GLACIAL	POST-GLACIAL		
UPPER	SARTANSKY GLACIATION	WEICHSEL: SALPAUSSELKA	WÜRM	WISCONSIN: VALDERS
	KARGINSKY INTERSTADIAL	WEICHSEL: ALLERÖD	WÜRM	WISCONSIN: TWO CREEKS
	ZYRYANSKY GLACIATION			
	KASANTSEVKY MARINE DEPOSITS			
	INTERGLACIAL: SANCHUGOVSKY	EEMIAN	R/W	SANGAMON
	INTERGLACIAL: MESSOVKY — BOREAL TRANSGRESSION	EEMIAN	R/W	SANGAMON
MIDDLE	MAXIMUM GLACIATION	SAALE	RISS	ILLINOIAN
	INTERGLACIAL	HOLSTEIN	M/R	YARMOUTH
LOWER	GLACIATION	ELSTER	MINDEL	KANSAN
	PRE-GLACIAL		GÜNZ	NEBRASKAN

Fig. 3. Siberian glacial chronology [after Saks and Strelkov (*22*)] and suggested correlations with glacial sequences in northern Europe, the Alps, and north-central North America.

"as fresh as well-frozen beef or horsemeat," only dogs showed any appetite for it; "the stench . . . was unbearable" (*1*, pp. 119, 129). Histological examination of fat and flesh of the Berezovka mammoth showed "deep penetrating chemical alteration as a result of the very slow decay," and even the frozen ground surrounding a mammoth had the same putrid odor, implying decay *before* freezing (*2*, p. 60). Furthermore, the stories of a banquet on the flesh of the Berezovka mammoth were "a hundred per cent invention" (*2*, p. 60).

Soft parts of other fossils are not unknown in the geologic record, but sudden or catastrophic changes of climate have not been postulated to explain the preservation of these parts. Skin and hair of Pleistocene ground sloths are known from nonglacial areas. From more remote times we have mummified skin of Mesozoic dinosaurs and muscle fibers of Devonian sharks, still showing individual fibers and cross-striations (*7*). Such fossil evidence implies preservation of these soft parts for a considerable period of time—at least as long as was required for lithification of the enclosing sediments.

The only direct evidence of the mode of death indicates that at least some of the frozen mammoths (and frozen woolly rhinoceroses as well) died of asphyxia, either by drowning or by being buried alive by a cave-in or mudflow. As stated above, sudden death is indicated by the robust condition of the animals and their full stomachs. Asphyxiation is indicated by the erection of the penis in the case of the Berezovka mammoth and by the blood vessels of the head of a woolly rhinoceros from the River Vilyui (Siberia), which were still filled with red, coagulated blood (*1*, *2*, *32*).

The specific nature of deposits enclosing the mammoths is not known well enough to be very helpful as an indicator of the mode of death or burial. Most of the remains are associated with river valleys and with fluviatile and terrestrial sediments, but whether the mammoths bogged down in marshy places or fell into "riparian gullies" or were mired in and slowly buried by sticky mudflows is not clear. Perhaps all three of these agencies and several others were involved. One point of fact helpful in this problem is the specificity of the frozen animals: in Siberia only mammoths and woolly rhinoceroses have been found frozen and preserved, and the former have been found in much greater numbers than the latter (*1*, p. 40; *33*).

So far no other members of the contemporary Eurasian fauna—stag, horse, reindeer, antelope, musk ox, and so on (*16*)—have been found frozen and well preserved. That only the bulky and awkward "giants" of the fauna are so preserved points to some peculiarity of their physique as a contributing factor. The low-slung rhinoceros would have trouble negotiating marshy ground and snow drifts. Similarly, the mammoth, with his stiff-legged mode of locomotion, would have difficulty on such terrain and, moreover, would probably not be able to cross even small gullies. It would be nearly impossible for him to extricate himself if he had fallen into a snow-filled gully or had been mired into boggy ground. A modern elephant is unable to pass over any trench which barely exceeds his maximum stride because of the pillar-like leg structure which is required to support his vast body (*11*, p. 41). Also, the mere weight of the mammoth's body would have been a dangerous attribute if the animal happened to graze too near the edge of a river bluff which had been softened by the summer sun.

The stomach contents of the frozen mammoths indicates that death occurred in the warm season, probably in late summer or early fall (*2*, p. 49; *18*), when melting and solifluction would have been at a maximum and, accordingly, locomotion would have been difficult.

The several theories of entombment, which have been alluded to above, generally reflect the theorist's particular experiences or impressions in the mammoth-bearing terrain. Digby (*1*) was impressed by "countless riparian gullies" which would have been ideal mammoth traps when filled with snow in the winter. Vollosovich (see *2*, p. 57) was himself trapped in a slowly moving stream of very sticky mud and had to be rescued by his guides. He theorized that an animal so trapped might fall on its side and act as a dam, being slowly buried and suffocated by mud. The Berezovka mammoth is commonly regarded (*12*) as having fallen as a cliff slumped beneath it; its broken bones attest to such a fall. Presumably it then suffocated as it was buried alive by the caving bluff. Popov (*20*) believes the Mamontova mammoth perished in a bog while grazing on the floodplain of the ancient Mamontova River. Quackenbush (*15*) believed that his specimen from Alaska perished on a floodplain and that most of the flesh rotted away before the corpse was naturally buried by floodplain sediments. Another possibility is drowning by breaking through river ice (*2*, p. 63). All of these theories are credible and can be accepted as possibilities. There appears to be no need to assume the occurrence of a catastrophe.

Conclusions

Although some problems concerning the frozen fauna of Siberia and Alaska remain to be solved, recent field work and new techniques have contributed much to our understanding since Tolmachoff's summary account in 1929. Frozen woolly mammoths have now been found in northern and northeastern Siberia and Alaska in deposits attributed to Last interglacial and Last glacial times. They are unknown in postglacial deposits. Only four of the 39 known frozen carcasses are by any means complete, and all of the cadavers were rotten and somewhat mutilated prior to being frozen. More than 50,000 mammoths lived in Siberia during late Pleistocene time.

The woolly mammoths lived in a tundra region similar to that in which they are found today, but the climate was slightly warmer and perhaps moister. They were apparently well adapted to the cold climate; their long hair, warm underwool, and thick layer of subcutaneous fat protected them against the cold air, and their broad, four-toed feet and relatively small size (as compared to that of their fossil European relatives) were advantageous in marshy pastures. The frozen mammoths were healthy and robust when they died.

The well-preserved specimens, with food in their stomachs and between their teeth, must have died suddenly, probably from asphyxia resulting from drowning in a lake or bog or from being buried alive by a mudflow or cave-in of a river bank. Since only the heavy-footed giants of the fauna—the mammoths and woolly rhinoceroses—have been found in a frozen state, it is very unlikely that a catastrophic congelation occurred in Siberia. On the contrary, the frozen giants are indicative of a normal and expected (uniformitarian) circumstance of life on the tundra (*34*).

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34. This article is Lamont Geological Observatory contribution No. 480. It was supported by a grant from the U.S. Steel Foundation. The subject was suggested and valuable advice was offered by M. Ewing and W. L. Donn of Lamont Geological Observatory. C. W. Hibbard and W. S. Benninghoff of the University of Michigan gave a great deal of help with biological details. H. E. Anthony and R. G. Van Gelder of the American Museum of Natural History were very helpful in supplying parts of the frozen baby mammoth for radiocarbon dating.

Debris from Tests of Nuclear Weapons

Activities roughly proportional to volume are found in particles examined by autoradiography and microscopy.

Jan Sisefsky

Since the summer of 1955, airborne debris from nuclear-weapon tests has been collected and examined at the Research Institute of National Defence, Stockholm. The debris has been collected on glass-fiber paper (30 by 60 cm) with a sampling device carried by aircraft (*1*) at altitudes up to about 13 kilometers. Usually two samples have been taken at the same time, one above and one below the tropopause. Debris has also been collected on glass-fiber paper at ground level.

Routinely, one-fourth of the filter paper is used for spectrometric determination of the contents of a number of gamma-ray-emitting nuclides, from which an age determination can be made (*2*). The remainder is autoradiographed on photographic film of the type most sensitive to beta rays—that is, no-screen x-ray film (*3*, p. 220) (Ilford "Ilfex"). The exposure time is usually 7 days, and the films are developed according to the recommended procedure (ID 42; 4 minutes, 20°C). The autoradiographs show black, dense, circular spots with diffuse edges, varying in diameter from a few millimeters down to about 10 microns (see Fig. 1); spots of 10-micron diameter are the smallest that can be distinguished. The largest spots appear only when there is fresh radioactivity; usually only a few of them appear per square decimeter of filter area. The smaller spots are more frequent, the smallest ones being sometimes so numerous that they merge to a black haze, reproducing the structure of the filter. Although, as a rule, samples taken above the tropopause are of higher activity than the corresponding ones taken below it, the ratio of big spots to small ones is usually larger for samples taken below the tropopause.

From the autoradiographs it is possible to determine the activity of the individual particles and to locate their position on the filter (see *4*).

Radioactive Particle Measurement

The simplest method of measuring the size of an autoradiograph spot is to determine its "diameter" under the microscope with a low-power objective (for example, × 4; ocular, × 12.5) and an ocular scale. The totally black center of a spot is surrounded by an area where the unexposed parts lie like islands in the blackening area. Further out from the center the black grains lie isolated, surrounded by unexposed film (Fig. 1). The edge of a spot can be defined as the zone where these two types of blacken-

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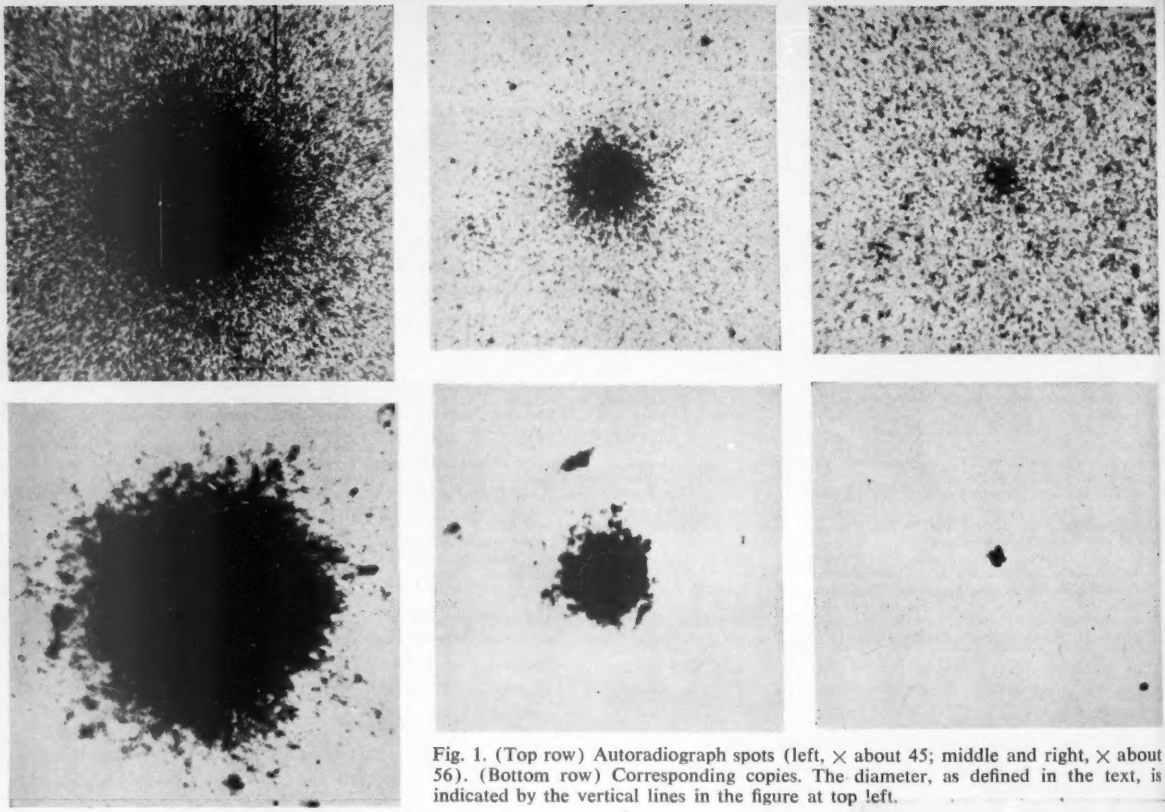

Fig. 1. (Top row) Autoradiograph spots (left, × about 45; middle and right, × about 56). (Bottom row) Corresponding copies. The diameter, as defined in the text, is indicated by the vertical lines in the figure at top left.

ing change. In this way, an approximate diameter of even very small spots can be defined. The reproducibility is about 5 to 10 percent for a spot exceeding 200 microns if the measurements are made by the same observer. Sizes of the smallest detectable spots (10 to 20 μ) can be estimated within a factor of 2. The spots may also be made more sharp-edged if they are copied onto high-contrast photographic film (Fig. 1). This process must be carefully standardized; otherwise each copy must be calibrated with standard spots.

Suppose, now, that there is a radioactive point source on a photographic emulsion film of negligible thickness. The radiation will diminish with increasing distance from the point source by at least the square of the distance, as the radiation is absorbed in the film. At a certain distance from the source, the radiation will be just strong enough to cause a blackening, characterizing the limiting zone defined above.

For a small spot we need consider only the β-radiation [the α-radiation of nuclear-weapon debris is negligibly weak and the γ-radiation has a blackening power only a few percent of that of the β-radiation (*3*, p. 18)]. Further, if we consider a spot of such small radius that the β-absorption in the emulsion and the change in the β-energy are negligible, we have in the edge zone of the spot:

$$S = K \cdot \frac{It}{d^2}$$

Therefore

$$d = K\,(It)^{\frac{1}{2}}$$

where S is the edge-zone blackening, I is the β-intensity, t is the exposure time, and d is the diameter of the spot, and K is a constant. If we plot the spot diameter versus the number of emitted β-particles on a log-log scale, the plot will start as a straight line with the slope

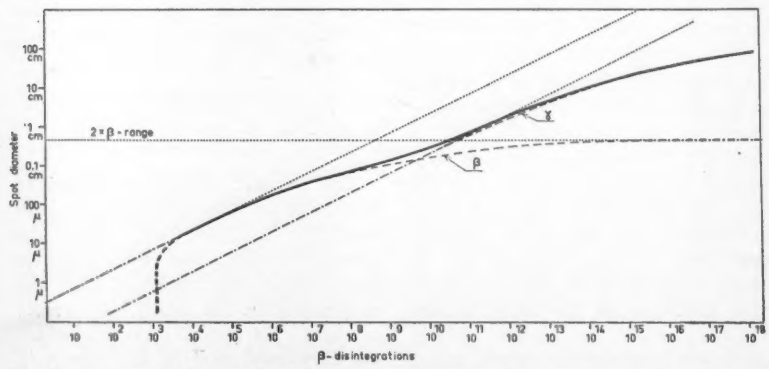

Fig. 2. An outline of a theoretical plot of autoradiograph spot size versus number of β-disintegrations, from a point source consisting of a single-spectrum β-emitter with a range of 0.8 g/cm² (corresponding to a maximal β-energy of 1.8 Mev); each β-particle is accompanied by one γ-quantum, for which the response of the x-ray film is 1 percent of the β-response (solid line). The dashed part of the line indicates the limit below which the spots are undetectable.

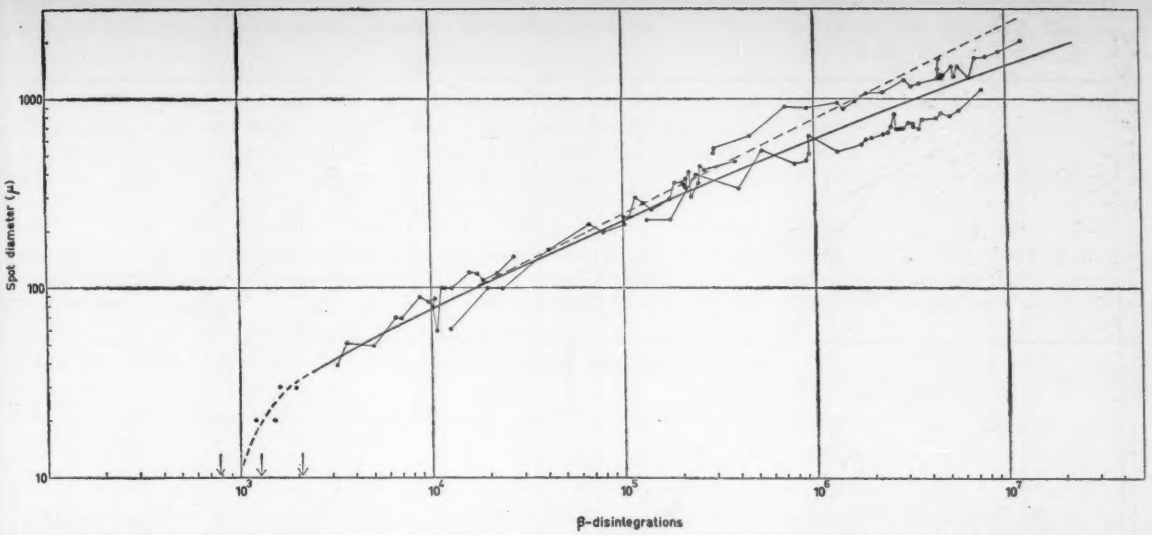

Fig. 3. Some typical plots of spot size versus activity for a set of nuclear-debris particles, showing the displacement effect (connected points) and the average plot (used for calibration purposes). The heavy line is compared with a line with the slope ½ (thin dotted line).

½. For larger spots we must consider the β-absorption in the emulsion as well as the change in the β-energy spectrum of the radiation. This will, of course, cause the curve to deviate from the straight line and to approach asymptotically the horizontal line corresponding to twice the maximum β-range for the radiation in question.

However, we must now also consider the γ-radiation, which will be predominant at long distances from the source. Since a monoenergetic γ-flux from a point source decreases approximately according to the law

$$I = \frac{I_0 \cdot e^{-\mu r}}{r^2}$$

where μ is the absorption coefficient and r is the distance from the source, the γ-radiation will cause the curve to rise again toward infinity but with a steadily decreasing slope (Fig. 2).

An attempt is being made to demonstrate these relationships by using different β- and γ-emitting nuclides as point sources. For the studies under discussion, nuclear-debris particles emitting radiation strong enough to be measured in β-counters (stronger than 10 to 20 disintegrations per minute) were used to determine the connection between activity and spot size. However, it turned out that the spot-size–activity plots were horizontally displaced from one x-ray film to another, even when the films were from the same box and the temperatures and processing times were carefully controlled (Fig. 3). This

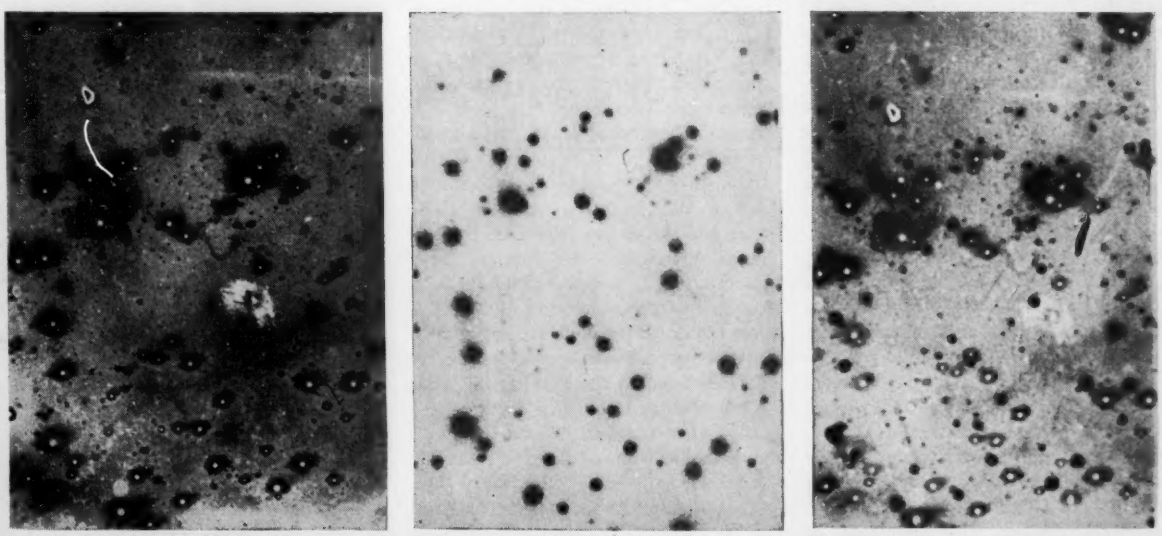

Fig. 4. (Left) A portion of a particle-plate coated with emulsion. The particles are found in the centers of the clear spots surrounded by dark rings. (Middle) The corresponding x-ray film autoradiograph. (Right) Copies of the left and center photographs superimposed, with a slight displacement. (About × 3.4)

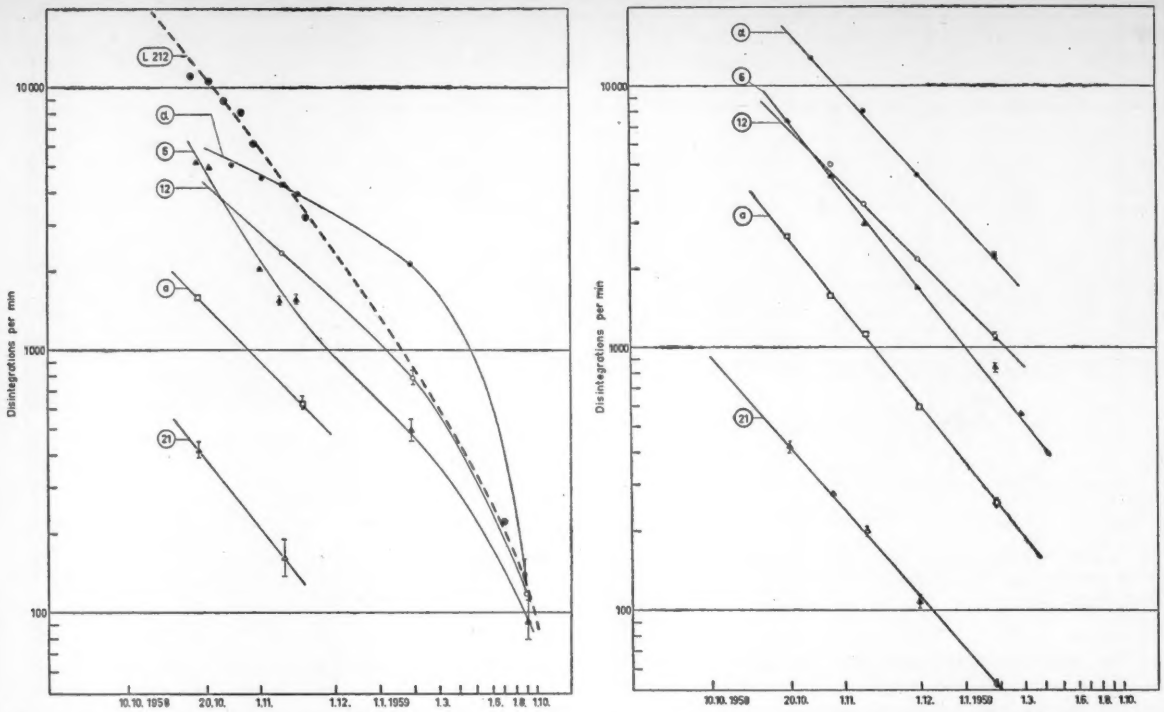

Fig. 5 (left). Some representative γ-decay curves, and a more anomalous curve (*6*) for single particles compared with the γ-decay curve for all radioactivity in sample L-212 (dotted line) sampled from 180 g of air. (Particles from sample L-211 are marked with letters; those from L-212 with numbers.) Gamma disintegrations per minute are calculated from γ-counts per minute simply by dividing by an average efficiency (7 percent), as the efficiency for the average energy of fission products probably does not vary more than 20 percent for the ages in question (*2*). Fig. 6 (right). Curves for β-decay for the same fallout particles as in Fig. 5.

made it necessary to normalize the plot for each film by means of β-measurements of a few particles.

By this method the β-activity of particles from nuclear debris can be estimated with an accuracy of about 10 to 20 percent down to approximately 3000 disintegrations during the exposure time; 1000 disintegrations are needed to produce a detectable spot (and the activity is then estimated by a factor of 2 to 3). If the exposure time is 1 week, the corresponding disintegrations per minute are 0.3 and 0.1, respectively.

It may be of interest to compare this method with ordinary β-measurements. A low-background anticoincidence β-counter, such as the one now in use in this laboratory (*5*), with 15 percent efficiency and background activity of 0.3 count per minute, requires 2 days to measure the activity of a particle with activity of 0.3 disintegration per minute with 20 percent accuracy. Since we were dealing with hundreds of particles with activity in the range of 0.1 to 10 disintegrations per minute, this procedure could not be used.

Microscopic Examination of the Particles

The method of microscopic examination used, described in detail elsewhere (*4*), is, briefly, as follows. The particles are transferred to celluloid films on glass plates. They are then covered with nuclear emulsion in gel form, which is "reversal-developed" by a special method that gives clear, circular autoradiograph spots against a brown transparent background. In the centers of these spots the active particles can be seen in the microscope. An ordinary x-ray autoradiograph is made simultaneously for use in activity determinations. (The clear circles on the plate are not suitable for this purpose because of their irregular size, due to the varying thickness of the hand-cast emulsion layer.)

If a great number of particles are to be examined on a plate, it may be profitable to transfer the x-ray film autoradiograph to the plate in the following way. The autoradiograph is copied onto a soft film (which gives a true negative without diminishing the sizes of the spots). This film is, in turn, contact-copied onto another film which should be of high contrast and thin (for example, Gevaert Dipos Contact). The copy, which shows the spots of the autoradiograph with greater contrast (at least for the smaller particles) is placed in close contact with the emulsion layer on the plate by means of immersion oil, so that the spots on the autoradiograph and the corresponding clear areas in the plate are adjacent to each other. The thin, clear film does not impede the examination of the active particles under

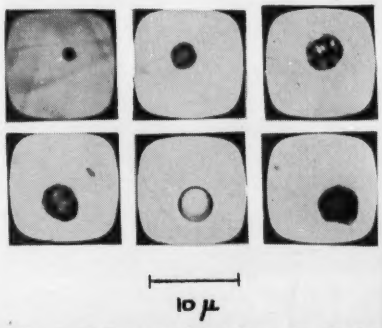

Fig. 7. Fallout particles from the Soviet bomb test of 30 September 1958.

the microscope, even if an immersion-oil objective is used (Fig. 4).

The particles are now visible under a microscope and can be identified by their positions in the clear areas (a low-power objective is used for larger areas). They are examined with the highest-power objective (oil objective, × 100; ocular, × 12.5), their diameters are measured by a calibrated ocular scale, and their color, form, and transparency are observed.

Soviet Bomb-Test Series of September-October 1958

The Soviet bomb-test series of September and October 1958 took place in Novaja Zemlja, beginning on 30 September with two explosions and continuing with several explosions during October. Seismic data (*6*) indicated that the most powerful explosions occurred on 18 and 22 October.

The radioactive debris was first observed by the Research Institute of National Defence in a ground air sample taken on 8 October 1958 in Stockholm. Nine days later, on 17 October, the debris was observed for the first time in a high-altitude sample. At a height of 12 kilometers over central Sweden a very strongly radioactive air sample (called L-212) was gathered (~ 30,000 pc/kg of air). (The count for the sample taken immediately prior to this, at the same height, was approximately

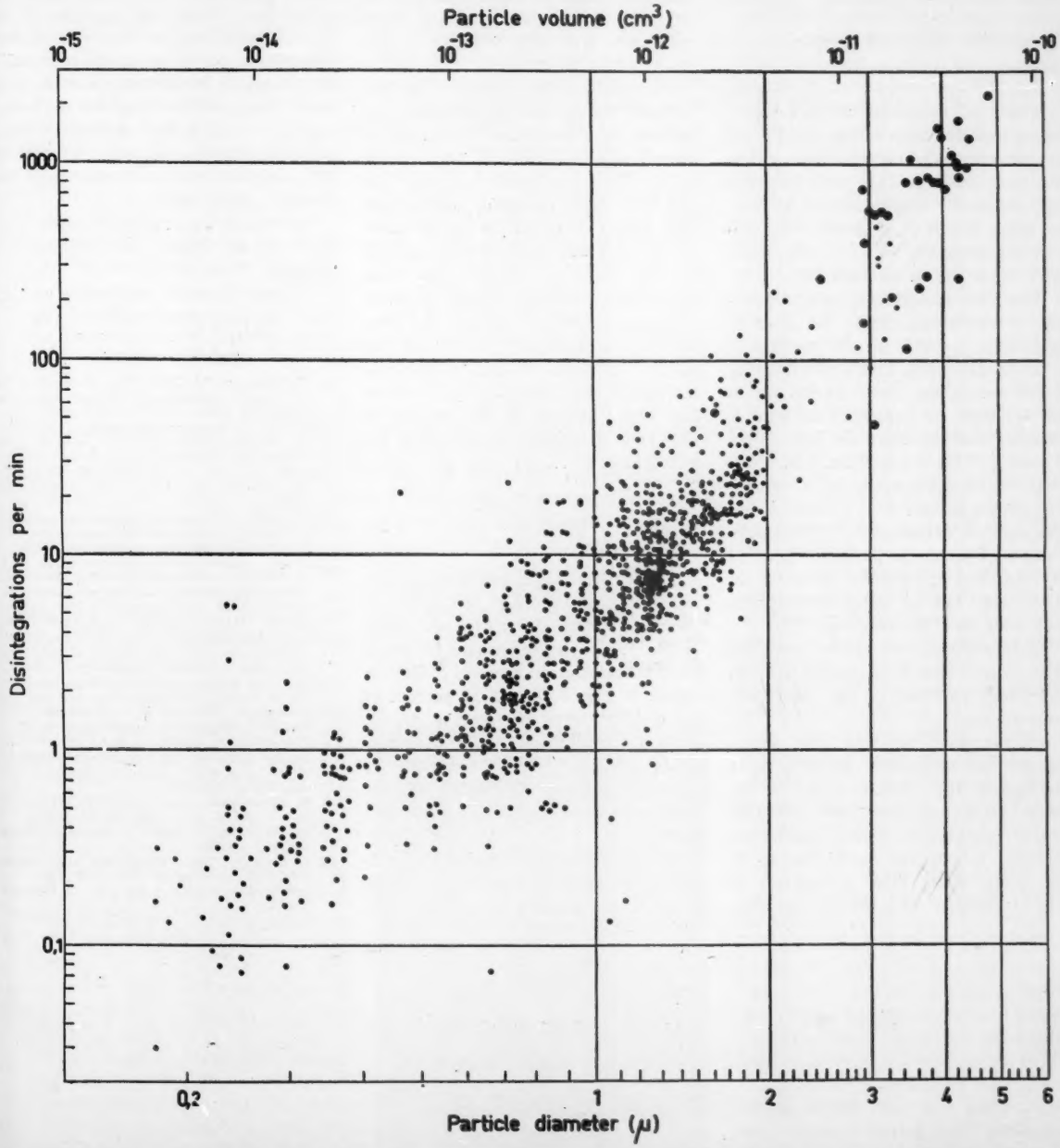

Fig. 8. Activity versus size for 950 particles from sample L-212 on 29 January 1958 at the age of 120 days. The particles measured in the β-counter are marked with the larger circles.

200 pc/kg of air.) In the high-altitude samples taken next, strong and weak ones alternated in an irregular way, but none comparable with sample L-212 was obtained. Sample L-212 could, from the γ-spectrometric measurements, be attributed to the tests of 30 September (*2*). The mixing with activity from earlier tests must have been insignificant, even for the weakest particles, and the sample was thus a very convenient one for study.

Examination of Debris from Explosion of 30 September

Thirty of the most strongly radioactive particles from sample L-212 and the accompanying sample from below the tropopause (L-211) were punched out, repeatedly measured with β- and, to some extent, γ-counters (*7*), and autoradiographed, with varying exposure times and at different ages.

The most strongly radioactive particles found, which caused the spots of millimeter diameter on the conventional autoradiographs, had a β-activity of a few nanocuries. Some of the curves for γ-decay are presented in Fig. 5, together with the curve for γ-decay for a sample of the whole filter.

In Fig. 6 the β-decay for a number of particles is plotted. It is obvious that this decay is of type $K \cdot t^{-k}$. The mean value for k for all the particles examined is 1.15, with a standard deviation of 0.15. This rate of decay corresponds well with the theoretical rate (*8*).

In addition to systematical variations with particle size (*9*) variations between individual particles of the same size were found.

In the plots of spot size versus activity no systematic variation with the age of the particles could be observed. Nor was there any indication that certain particles with an anomalous activity composition would fail to fit the plots. Some plots are shown in Fig. 3 together with the average plot, which is used as a standard curve for the activity measurements. When the standard curve is used, some strongly radioactive particles of a sample are both measured in a β-counter and autoradiographed. The standard plot is then displaced so as to give the best adaptation to the plotted points of these particles for each film. A small systematic error may arise for the less radioactive particles, as they do not have the same composition as the more radioactive ones (*9*). (According to the foregoing theory, the lower parts of the curve are not very sensitive to the energy of the β-particles.)

The relationship between particle size and activity was investigated, and microscopic examination was made of about a thousand particles from sample L-212. The great particle density on this filter made punching unnecessary; filter pieces of about 20 square centimeters each were leached with acetone, and the main part of the glass fibers was filtered away by a metal gauze of 200-micron mesh. Although the fiber residual retained 15 percent of the activity, it could be shown by autoradiograph that this activity contained less than 1 percent of the particles of sizes large enough to be seen under the microscope (>0.2 μ).

Results

The particles are colorless to reddish, translucent, and more or less spherical. Some characteristic particles are shown in Fig. 7. Activity versus size for about 1000 particles at an age of 120 days is shown in Fig. 8. About 30 percent of the particles were measured at an age of 45 days and projections of the measurements to 120 days were made by using the average disintegration slope for the 30 chosen particles mentioned above.

On account of variation in efficiency of the filter with particle size, the plot does not give a true picture of the particle size distribution, even if the 30 particles with strong β-activity and the lower part of the plot, where a great fraction of the particles in the sample are not represented, are excluded.

If we frame the "point-cloud" in Fig. 8 by two lines with the slope lying at a vertical distance representing a tenfold increase in activity, chosen by visual adapting, the cloud can be characterized by the symmetrical line between these two framing lines. Thus we have, for 120-day-old particles, $I = 6.7\ d^3$, when I is the β-activity in disintegrations per minute and d is the particle diameter in microns. Fourteen percent of the particles lie outside the framing lines—that is, the activities of these particles differ by more than a factor of 10^1 from the activities derived with the formula given above.

By using the decay formula $I = K \cdot t^{-1.15}$, we have, for 100-day-old particles, $I = 8.4\ d^3$.

A more detailed description of this work has been given elsewhere (*10*).

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Bernard O. Dodge, Mycologist, Plant Pathologist

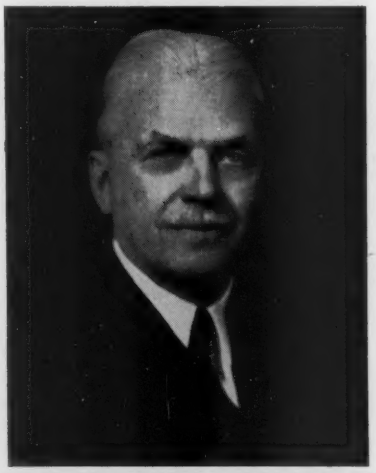
Bernard Ogilvie Dodge was born 18 April 1872 on his father's farm near Mauston, Wisconsin. He died at the age of 88, on 9 August 1960, in St. Luke's Hospital, New York City.

His ancestors were old New England stock, mainly of English origin. They were farmers, blacksmiths, tanners, wheelwrights. His paternal grandfather, Levi Dodge, was descended from Richard Dodge, who came from Somersetshire, England, to Salem, Massachusetts, in 1638. His paternal grandmother, Olive Blanchard Dodge, was the granddaughter of John Blanchard, born in Connecticut in 1737 and probably descended from Blanchards who were in Massachusetts in 1639. His maternal grandfather, William Nourse, was a lineal descendant of Francis Nourse, who came from England and was in Salem in 1639. Francis Nourse's wife, Rebecca Towne Nourse, had come to the Massachusetts Bay Colony from Great Yarmouth, England, about 1638. During the hysteria which swept Salem in 1692, Rebecca Nourse was accused of witchcraft and on 19 July 1692, at the age of 71, was hanged on Gallows Hill. Dodge's maternal grandmother, Mary Ann Rogers Nourse was descended from Robert Rogers, who came from England to the Massachusetts Bay Colony in 1634.

Dodge's father, Elbridge Gerry Dodge, was a carpenter by trade, but he devoted most of his active life to farming and to teaching in the local schools. A man with no high school or college education, he had a wide acquaintance with English literature and on occasion would recite entire scenes from Shakespeare to his attentive family.

Dodge's mother, Mary Ann Nourse Dodge was 16 when she accompanied her family from Vermont to Wisconsin, in 1856. She taught school until she married Elbridge Gerry Dodge before a justice of the peace in 1861, in spite of the strong disapproval of her father. Mary Ann Dodge bore her husband five sons and two daughters and lived the life of a farmer's wife. She loved music, wrote verse (though she had no more schooling than that provided by a district school), and at 69 undertook the translation of a 64-page Spanish story into English. Dodge's mother and father were happy in their married life; they loved literature and respected learning —a circumstance which must have had much influence on their son.

Bernard Dodge spent his young manhood on his father's farm and did not complete his high school education until he was 20. He then taught school for several years. He entered the University of Wisconsin as a special student in 1895, but before the college year was completed his funds were exhausted and he found it necessary to return to teaching. After alternating periods as teacher and student, he graduated from the Milwaukee Normal School in 1901. He then spent five summer sessions and the school year of 1908–09 at the University of Wisconsin and received the degree of bachelor of philosophy in 1909, at the age of 39. He married Jennie S. Perry in 1906.



Bernard Ogilvie Dodge. [Bachrach]

Dodge became interested in the fungi while he was a high school teacher in Wisconsin, through a chance meeting with an elderly Bohemian tailor who was collecting edible fungi. He maintained his interest at the University of Wisconsin and continued to be fascinated by this group of plants all his life.

After completing the requirements for his undergraduate degree, Dodge accepted a position as assistant and research fellow in botany at Columbia University and received the degree of doctor of philosophy in 1912. He remained at Columbia as an instructor until 1920, when, on being appointed plant pathologist (in fruit diseases) in the Bureau of Plant Industry of the United States Department of Agriculture, he moved to Washington, D.C. In 1928 the New York Botanical Garden appointed him plant pathologist, and he retained that position until he retired in 1947, to become plant pathologist emeritus and consultant in mycology. At his request, he was relieved of his duties as consultant in 1957, but he continued to work in his laboratory until a few months before his final illness, making his way several times each week by subway to the New York Botanical Garden, nearly an hour's journey from his apartment in the vicinity of Columbia University.

Dodge published about 150 papers dealing with the life histories, cytology, morphology, pathology, and genetics of the fungi and with insects and other animal pests of plants. He was 42 when his first paper was published; his last appeared when he was 85.

As a graduate student and instructor at Columbia University, Dodge's first interest was the Ascobolaceae, especially their reproduction; later, as plant pathologist in the Bureau of Plant Industry, he was concerned with the rusts, and his investigations dealt with diseases of raspberries, blackberries, and dewberries.

Toward the end of his stay in Washington his attention was directed to *Neurospora*, and in 1927 he published the first of 42 papers on this fungus, which remained a major concern for the balance of his life. He described its life history, distinguished three genera, produced species hybrids, clarified the mechanism of mating type inheritance, demonstrated the Mendelian nature of inheritance in this fungus, obtained ab-

solute Mendelian ratios by isolating and growing the lineally arranged ascospores of a single ascus, used the fungus to define the phenomenon of "heterokaryotic vigor," and made many other fundamental contributions to the biology and genetics of this organism. In many respects Dodge's discoveries and views were well in advance of those of his time, and his findings had an important influence on the development of genetics, especially of microorganisms, and on biochemical genetics, for which *Neurospora* became so useful a tool.

He convinced T. H. Morgan of the potentialities of *Neurospora*, and Morgan took cultures with him when he moved from Columbia University to the California Institute of Technology in 1928. There, C. C. Lindegren, on Morgan's advice, undertook the preparation of a dissertation on *Neurospora* and spent the summer of 1930 in Dodge's laboratory, one of many students and colleagues who felt the impact of Dodge's ideas and enthusiasm.

At the New York Botanical Garden Dodge was responsible for the control of plant diseases and pests, a responsibility which he took very seriously, frequently supervising in person the spraying, dusting, and other control measures. He published research findings and observations on diseases of iris, Japanese cherries, pachysandra, roses, geraniums, cedars, marigolds, opuntias, delphiniums, and other ornamental plants and a text on diseases of ornamental plants.

His researches on *Neurospora* were an "extra," pursued while he carried on his official duties of plant pathologist and coped with limitations of time, assistance, and facilities uncommon in these days of substantial support of basic research. Routine media were frequently prepared by Dodge himself; individual ascospores were isolated by means of a sharpened sewing needle inserted in a simple wooden handle. It is, of course, fruitless to speculate on what he might have accomplished had he been less handicapped by lack of funds in his earlier years and more generously supported in his research later on.

Dodge's concern with fungi as causes of disease extended from plants to animals and man. From 1928 to 1939 he was consultant in mycology for the Presbyterian Hospital, New York City, and from 1929 on, was lecturer in dermatology for the College of Physicians and Surgeons, New York.

In his later years Dodge was intrigued by phyllotaxy, the Fibonacei series, and other expressions of design in nature, and with his usual enthusiasm he spent much of his spare time puzzling over them and directing the attention of his colleagues to their wonders.

Dodge's contributions to science were widely recognized. He was a member of Sigma Xi and of the National Academy of Sciences, fellow of the American Academy of Arts and Sciences and of the American Association for the Advancement of Science, foreign member of the Linnean Society of London, and honorary member of the British Mycological Society. He received the Distinguished Service award of the New York Botanical Garden and the Golden Jubilee award of the Botanical Society of America.

Dodge participated willingly and effectively in the affairs of the various organizations to which he belonged. He served as associate editor of *Mycologia*; editor of the *Bulletin of the Torrey Botanical Club*; convener of the section on fungi and fungus diseases of the 3rd International Microbiological Congress; secretary-treasurer and president of the Torrey Botanical Club; president of the Mycological Society of America; vice-president of the 7th International Botanical Congress; and vice-president of the American Association for the Advancement of Science.

He was a big man physically as well as mentally. Blond, blue-eyed, fine-looking, he was over six feet tall and weighed 190 pounds. He was modest to an extreme, a bit shy, friendly, cheerful, good-natured, never bitter, and especially marked by his enthusiasm. Nothing pleased him more than to inspire in some beginner or colleague enthusiasm for the subject in which he took so great an interest, and few could resist him. He was inclined to be conservative in politics and was affiliated with the Episcopal church.

He is survived by his wife, Jennie Perry Dodge.

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Science in the News

Educating the Public: To Win Broad Support for His Program Kennedy Assumes the Role of Mass Educator

Last week the Administration's education program had become so enmeshed in the side issues of civil rights and religion that Chairman Powell, of the House Education and Labor Committee, announced that unless the situation could be cleared up, the President's program was dead. At the same time it had become clear that the Administration's over-all program, including its plans for science, had become enmeshed in an education problem of another and more general sort: the problem the Administration faces in educating the country as a whole to what it is talking about.

The paradox with which the Administration is faced is that, although Kennedy himself has made an exceedingly good impression on the country (a Gallup poll taken after his first 30 days in office showed him enjoying a popularity even greater than Eisenhower's at the same point of his Administration), nevertheless there seems to be no discernible ground swell of support for his program. Few people doubt either that if the election were to be held over again this week, Kennedy would beat Nixon by a much greater margin than he did in November, or that if Congress were to vote on

his program this week, large parts of it would be defeated.

The problem Kennedy faces is not merely convincing the country that his proposals to meet problems are sound but the deeper one of educating the country to an awareness of what the problems are. The central issue, and the one in which the problem of public education is clearest, is that of economics. Kennedy's message on budget revisions is not due until next week, but his over-all requests will apparently total at least $5 billion more than was asked for in Eisenhower's budget. And this is only a beginning, for, particularly in the areas of education and natural resources, many of the proposals contain built-in increases for succeeding years.

The cost of the education program, if enacted, would, for example, double in three years, from $770 million in fiscal 1962, beginning this June, to $1.5 billion in fiscal 1964.

Even this is not all, for what the Administration has asked for so far is by no means all it feels it must ask for eventually, a fact that is suggested by Kennedy's repeated description of his programs as "modest." Large areas of the Science Advisory Committee's recommendations for support of basic research and higher education, for example, have not been dealt with so far (". . . the Federal Government has no escape. Either it will find the policies—and the resources," the committee report said, "or no one will"). Kennedy's confidence in the men who made the recommendations is reflected in his appointment of Glenn Seaborg, who headed the panel which produced the report, to be chairman of the Atomic Energy Commission, and in his bringing McGeorge Bundy, the Harvard dean who was largely responsible for getting the committee's ideas down on paper in clear and forceful language, into the White House as a special assistant to the President.

Kennedy's Appointments

There is no reason to doubt that the President will move to put the view of the Seaborg panel into effect bit by bit, and this is only one of a number of sets of recommendations in which Kennedy has demonstrated his interest in very concrete fashion by putting their exponents into influential positions in his Administration.

Under Eisenhower, the budget, after an initial decline made possible mainly by the ending of the Korean War, grew at a rate of about $2 billion a year. Kennedy's program would require a rate of growth a good deal higher than the Eisenhower average; this makes his policies for economic growth not just one area of the Administration program but the central area, which, if the Administration's economic views are correct, will make the rest of the program possible.

Terms of Debate

Yet Kennedy has not so far succeeded in getting his Administration's view of the nature of the economic problem across to the public, although an initial effort to do so has been made. To date, despite the Administration's efforts, the debate over what to do about the economy has been dominated by the question of what to do about the current recession, although the current recession is not at all what the Administration is really concerned about, and a debate on these grounds is one the Administration is almost certain to lose by.

The Administration so far has not succeeded in convincing many people who were not already convinced before Kennedy took office that it is not enough for the economy to be going up, but that the *rate* at which it is going up is extremely important.

Problem of Growth

The Administration's view accepts the need to raise this rate of growth from the 2.5 percent we have averaged since 1953 to at least 3.5 percent, diagnoses the basic weakness in the economy as an insufficient demand for goods and services, and proposes to make up for the slack demand by running an unbalanced budget to pump extra money into the economy until it is completely back on its feet. The reasoning behind this was outlined here several weeks ago in a report on Kennedy's economics.

If the right to vote were limited to people with Ph.D.'s in economics, there is not much doubt that Kennedy would easily win heavy, although not unanimous, support for his views. If the Administration had a free hand to go ahead with its policies for several years and then ask the public to judge the results, it would, as Kennedy likes to say, "look to the future with some confidence." Since neither is the case, the Administration has a serious problem.

The Kennedy view is not a radical one. Deficit spending to stop a recession has become a conventional remedy, as was demonstrated by the almost complete lack of any outcry when the Administration announced that it would consider a temporary tax cut if the recession proved worse than expected. What has not become conventional is the notion that stimulation of the economy is justified not only when the economy is in a clearcut decline but also whenever excessive slack appears, as measured, perhaps, by the rate of unemployment.

There have been signs of change, as when the Committee for Economic Development, made up of about 1200 business executives and university administrators, sponsored testimony before the Joint Economic Committee of Congress criticizing Eisenhower for balancing the budget in 1959, after the 1958 recession was over but before a vigorous recovery had been achieved. In the same vein, and before the same committee, Kennedy's Secretary of the Treasury, Douglas Dillion, said, "The problems of bringing about a prompt recovery, and more importantly, vigorous expansion, call for the stimulating potential of a larger Government budget. . . ." What is significant in such testimony is not the advocacy of government spending to stop a recession, which has become a conventional, if still widely unpalatable medicine, but the advocacy of government spending beyond the point where the economy has turned up again, in order to promote a vigorous recovery.

Congressman Thomas Curtis, a member of the Joint Economic Committee, who has become a leading critic of Kennedy's economic policies, argues, as his central point, that it is unreasonable to expect post-1953 rates of growth to be as high as pre-1953 rates, because the Korean War stimulated the economy before 1953. "You have to distinguish," he says, "between a war economy and a peace economy." "It should be common knowledge that the rate of economic growth measured in GNP can be stimulated by war and that it will relapse in peacetime." The Kennedy people see this, though, as an argument for their point of view: Why should we need a war, they ask, to have a vigorous economy? If spending for war is so effective in picking up the slack in the economy, then why not extra spending for schools and slum clearance and the rest of the Administration program to take up the slack during the weak periods of the economy in peacetime?

But there is a great difference, at least in terms of political tactics. When there is a war there is no argument about the need to spend the money, and therefore no occasion for justifying or opposing the spending in terms of what it might do for or to the economy.

During a recession, declining tax revenues produce a deficit whether the Administration or Congress wants it or not, and the only question is the size of the deficit.

The problem Kennedy faces is in asking for a deficit neither in war nor in recession but in fiscal 1962 and 1963, when the economy, it is assumed, will be on the upswing. To win support he must convince the country that the combination of the innate value of the programs on which the money will be spent and the importance of pushing economic growth add up to as compelling an argument for more spending as either the needs for arms programs in wartime or for government stimulation during an actual recession provide by themselves. This implies a great effort at public education. Walter Lippmann recently wrote a column arguing that the President must not only undertake to be the national leader but the national teacher, and the White House has let it be known that Kennedy will begin this task in earnest through a series of television broadcasts to begin within a few weeks.

The difficulty of the task must be measured by the degree of public understanding that already exists. Congressman Curtis' lengthy speech in the House attacking Kennedy's economics, for example, began with a demonstration that the rate of growth under Eisenhower was 4.6 percent a year rather than 2.5 percent a year as Kennedy has claimed.

Curtis arrived at this conclusion by the simple device of not bothering to take into account price inflation. He was aware of this, but apparently saw nothing wrong with it, for he describes the method by which Kennedy arrived at the lower 2.5 percent in a context that implies there was something sneaky about the way Kennedy's figures were adjusted to take into account price inflation.

What was more curious than Curtis' argument, which might be dismissed as questionable but widely used political tactics, was that a widely published middle of the road reporter wrote a column praising Curtis' "brilliant" presentation, and in particular his demonstration that the rate of growth under Eisenhower was higher than Kennedy had said it was.

In a similar vein, Senator Goldwater recently argued that the Administration was exaggerating the recession. The Senator pointed out that although unemployment normally rises in January, contrary to the normal pattern the seasonably adjusted unemployment rate had actually declined this January. Unemployment had in fact gone up by a million in January, but Goldwater apparently did not choose to recognize the meaning of the term "seasonally adjusted."

This does not mean that there are not good arguments against Kennedy's proposals, especially philosophical ones dealing with the proper role of the government in a free society. But the frequency with which responsible figures make arguments that would flunk them in a freshman course in economics suggests the difficulty the Administration faces in getting across to the public the reasoning behind his proposals.

The Administration's greatest problem in economic policy is one that it faces almost everywhere in its program, and particularly in education. There is a wide gap between the ease with which it can get across the idea that something should be done and the ease with which it can get across the idea that although we are already doing a good deal we should be doing still more.

In economics, the Administration needs to get across the basis for proposing stimulation when the economy is growing and reaching new highs of output. This is a difficult notion to put across, while the contrary argument of why tinker with a system that produces new record highs almost every year is a very simple argument.

On another problem, the Administration's team of negotiators set off for the Geneva talks, resuming the 21st, after a round of luncheons and briefings intended to convince the Senate Foreign Relations and Atomic Energy Committees that the U.S. position at Geneva would be based on a tough-minded balancing of the risks involved in either reaching or failing to reach agreement, and that, therefore, if agreement is reached with the Russians, the Senate would ratify the treaty. Word leaking out of the briefings suggested that the effort was not entirely successful.—H.M.

News Notes

Biologists Speak Out Against Bills To Regulate Animal Use

Many American biologists have expressed opposition to proposed federal legislation to regulate research in which experimental animals are used. Their position is stated in the February *Bulletin* of the American Institute of Biological Sciences, in an article by Hiden Cox, the editor.

Four bills, the Cooper Bill in the Senate and three identical House bills, were introduced in the 86th Congress. None was passed. A similar bill, H.R. 1937, has been introduced in the 87th Congress by Representative Griffiths (D-Mich.). The Cooper Bill provided in its preamble for "the humane treatment of animals used in experiments and tests by recipients of grants from the United States and by agencies and instrumentalities of the United States Government. . . ."

The *Bulletin* article reports the reactions to the bill of a number of biologists and biological organizations. The *Bulletin* itself takes no stand on the legislation, but Cox explains the attention given the measure in the journal in these words:

"To the surprise of few, the Cooper Bill was greeted by something less than unanimous enthusiasm. All biologists, directly or indirectly, are affected by provisions of this bill. Since this or any similar bill is aimed straightway at the research programs of a large share of biologists, the *Bulletin* should provide a means by which biologists can have their say. . . ."

The article begins by quoting a resolution adopted on 28 October 1960 by the Animal Care Panel, an organization of veterinarians, scientists, and others. The resolution declared that the bills introduced in the 86th Congress would "permit an unwarranted encroachment upon the research worker's freedom . . . delay the testing of new concepts and ideas and would hinder and restrict medical and biological research. . . ." The resolution also said that the bills contain "no constructive provisions to deal with the real current needs in the field of laboratory animal care" and would "retard the objective of rational humane care for laboratory animals."

Maurice Visscher of the University of Minnesota, former president of the

American Physiological Society, comments that the Cooper bill provided, "according to a strict interpretation," that living vertebrate animals may not be used for teaching purposes, to increase knowledge in pure science, or even in agriculture or animal husbandry, but only for medical and military purposes.

One section of the bill, he observes, "sets up an absurd regulation, namely that all animals used for surgical training be sacrificed before the most important results of the surgery are known. Survival of the patient is the most important objective in surgery and only a fanatic would want to deprive young surgeons of opportunity to learn how to accomplish this." Visscher also says that, although anticruelty laws exist, "no professional scientist has ever been convicted of cruelty in relation to a scientific laboratory experiment."

A. B. Otis, professor and head of the department of physiology at the J. Hillis Miller Health Center of the University of Florida, points out that legislation such as the Cooper bill "would add significantly to the cost of administering government-supported research, and the amount of paper work involved . . . would hamper productivity of medical research." The bill, he says, "provides for nothing positive in the way of education or research that would lead to continuing improvement in the care of animals."

Lester R. Dragstedt, Chicago, president of the National Society for Medical Research, comments: "It is not reasonable to assume that police inspectors could be hired by the Secretary of Health, Education and Welfare who would be wiser, kinder and better qualified technically to supervise the conduct of scientific research than are the university presidents, deans of medical schools, directors of research institutes and academic department heads who now bear the responsibility for the character of animal research in the United States." If the Cooper bill were strictly interpreted, Dragstedt writes, "this would stop all medical and biological research except [that] on plants and microbes for thousands of years until scientists could be sure that every possibility for the use of such lower forms of life in the solution of medical problems has been exhausted. . . ."

Cox notes in concluding his article that last year a subcommittee of the AIBS Committee on Education, headed by G. Ladd Prosser of the University of Illinois, issued a code for use of animals in high school biology courses. Cox cites it as "one of many evidences" that scientists "are and long have been concerned with humane treatment of laboratory animals."

Land-Grant Colleges Commemorate Founding, Plan for the Future

Sixty-eight American colleges and universities are preparing to celebrate a centennial—the establishment of the land-grant system of higher education. The actual anniversary is 2 July 1962, but a steering committee of educators already has scheduled a series of programs for 1961 and 1962. At centennial headquarters in Washington, D.C., arrangements have been made for nation-wide conferences, seminars, and exhibits to focus attention on the origin of the institutions and on their contributions to the economic, cultural, and scientific growth of the United States.

Established during the Civil War

The land-grant system, which was developed in the Civil War period, was based on the concept that learning should be practical as well as academic, and that it should be open to all, not just to a privileged few.

Congressional legislation to that end was first drawn up by Representative Justin Smith Morrill of Vermont. In final form, it provided for a federal grant of land to each state, apportioned at the rate of 30,000 acres for every senator and representative. With money from sale of the land, each state was to establish and endow "at least one college where the leading object shall be, without excluding other scientific and classical studies, and including military tactics, to teach such branches of learning as are related to agriculture and the mechanic arts."

Forward-looking men supported the Morrill bill in the belief that widespread knowledge and training were essential to the nation's agricultural and industrial progress. But opposition was heavy. Though passed by a small majority in 1859, the measure was vetoed by President Buchanan on grounds of expense, potential land speculation, competition with existing universities, and infringement of states' rights.

A similar bill was passed and signed into law by President Lincoln on 2 July 1862. Its support owed much to the provisions for military training; the lack of such training had been painfully apparent in the North's losses earlier in 1862.

Land-Grant Institutions Today

Today land-grant institutions flourish in all 50 states and Puerto Rico. Twenty percent of all U.S. college students are enrolled in them. These schools confer all the doctoral degrees awarded in agriculture throughout the nation, about half of those awarded in sciences, engineering, and health professions, and a quarter of those awarded in arts, languages, business, and education. To these institutions goes much of the credit for extending the teaching of the humanities throughout the United States.

The colleges and universities that grew out of the Morrill legislation now play a vital role in programs providing American technical aid abroad. Through the Reserve Officers Training Corps, they also prepare nearly half the civilian-trained regular and reserve officers of the armed forces.

The land-grant institutions are responsible for advances in farming methods, new drugs to treat disease, and basic studies of raw materials used in industry. Their pioneering research in atomic-age sciences produced the first cyclotron.

Leaders of the Land-Grant Centennial plan to open discussions that are expected to have far-reaching effects on the nation's future.

Science Foundation Announces Summer Training Programs for High School Students

More than 9000 secondary school students of superior ability are being given the opportunity, by the National Science Foundation, to obtain tuition-free science training during the summer of 1961. The foundation has announced the award of 180 grants, amounting to $2 million, for summer science training programs for secondary school students of high ability. A total of 158 colleges, universities, and nonprofit research organizations are developing special programs and using their faculties to provide instruction.

Programs being offered are of two main types. One stresses lectures, quizzes, supervised study periods, laboratory work, and field trips centered around one or more areas of science. The other gives the student real research experience by enabling him to work on a

project of appropriate scope under the guidance of experienced scientists. Some programs combine elements of both types.

New Program Included

A third type, the Cooperative College-School Science Program, new and experimental in nature, is being included this year, with 19 programs at 17 institutions. It is designed to further the development of more rigorous curricula in secondary school science and mathematics. It will provide advanced training for about 2700 talented secondary school students and 50 secondary school teachers.

Admission to the training programs will be determined by the sponsoring institution, not by the National Science Foundation. In general, selection will be based upon scholastic ability, scientific motivation, and the completion of specified high school courses in science and mathematics. Students now in the 10th, 11th, and 12th grades are eligible. It is expected that the great majority of those selected will have completed their sophomore or junior years.

Partial support for participation will be available, so that students whose families cannot afford to pay all expenses for attendance at an institution will not be penalized. Students will be expected to meet as much of the expense as possible.

A list of the institutions where training programs for secondary school students will be held this summer, the name of the director at the institution, and the kind of program offered may be obtained from the National Science Foundation, Washington 25, D.C.

News Briefs

President's committee. The new members of the President's Science Advisory Committee, which is under the chairmanship of Jerome Wiesner, are Gerald R. Zacharias of Massachusetts Institute of Technology, Paul M. Doty of Harvard University, Harold Brown of the University of California's Lawrence Radiation Laboratory, and Frank Press of California Institute of Technology.

Members of the committee appointed by the previous Administration who have been asked to complete their terms are Detlev W. Bronk, Walter H. Zinn, George B. Kistiakowsky, Harvey Brooks, Wolfgang Panofsky, Donald F. Hornig, John W. Tukey, Robert F. Loeb, Alvin M. Weinberg, John Bardeen, and Emmanuel R. Piore.

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Biology program in Costa Rica. The department of biology of the University of Southern California has announced that an advanced biology institute for junior college, college, and university instructors, entitled "Fundamentals of Tropical Biology: An Ecological Approach," will be held in Costa Rica, with the cooperation of the Universidad de Costa Rica, 3 July–11 August. The project is supported by the National Science Foundation. Headquarters of the institute will be at the university, in San José, but there will be 10 days of field work and instruction, in English, in all major tropical habitats within the country.

The project is headed by J. M. Savage and Andrew Starrett of the University of Southern California and Rafael L. Rodríguez of the university in San José. The institute is open to any teacher of biological sciences, whether or not he or she has been concerned with biology of the tropics. Participants will receive travel expenses and a per diem allowance. For information, write to: Jay M. Savage, Department of Biology, University of Southern California, Los Angeles 7, Calif.

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1962 Limnology congress. The 15th International Congress of Limnology will be held at the University of Wisconsin, 20–25 August 1962. The congress is sponsored in North America by the American Society of Limnology and Oceanography and cosponsored by the National Academy of Sciences–National Research Council and the International Society of Theoretical and Applied Limnology. The congresses are held triennially, and all previous congresses have been held in Europe. Inquiries should be addressed to: The XVth International Congress of Limnology, Dr. John C. Wright, Birge Hall, University of Wisconsin, Madison 6.

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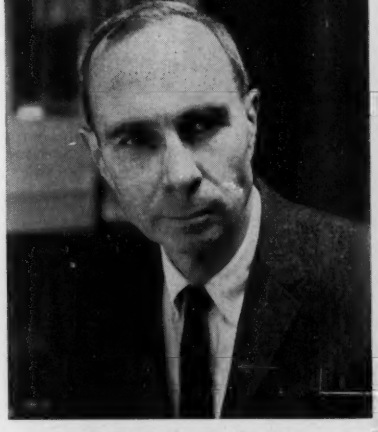
Medical electronics. The 4th International Conference on Medical Electronics, in combination with the 14th Annual Conference on Electrical Techniques in Medicine and Biology, will be held at the Waldorf-Astoria, New York, N.Y., 16–21 July. The conference is sponsored by the Joint Executive Committee on Medicine and Biology (IRE, AIEE, ISA), under the auspices of the International Federation for Medical Electronics. It is organized by the Institute of Radio Engineers through its Professional Group on Bio-Medical Electronics.

Anyone wishing to participate should submit an abstract of 300 words for preliminary review and a 50-word summary of his contribution for inclusion in an advance program; these should be sent *before 1 April* to Dr. Herman P. Schwan, Program Chairman, Moore School of Electrical Engineering, University of Pennsylvania, Philadelphia 4, Pa. Abstracts and digests may be in English, French, German, or Russian.

Scientists in the News

Philip J. Darlington, Jr., of the Museum of Comparative Zoology at Harvard University, has won the National Academy of Sciences Daniel Giraud Elliot Medal for his 675-page study, *Zoogeography: The Geographical Distribution of Animals.* The medal, awarded for the most meritorious work in zoology or paleontology published during a previous year, will be presented during the annual meeting of the academy in Washington on 24 April.

A member of the museum staff since 1932, Darlington is now Fall curator of Coleoptera and curator of insects. He is especially known for his taxonomic work on ground beetles and for his study of the distribution of animals over the earth. For some 30 years he has collected insects, birds, amphibians, and reptiles for the museum in Australia and the West Indies.



Philip J. Darlington, Jr.

Among the six outstanding career civil servants who have been selected to receive the 1960–1961 Rockefeller Public Service Awards of $3500 each are the following:

Sterling B. Hendricks, chief scientist, Mineral Nutrition Laboratory for Pioneering Research, Agricultural Research Service, U.S. Department of Agriculture (in the area of science and technology).

Richard E. McArdle, chief, Forest Service, U.S. Department of Agriculture (in the area of conservation and resources).

Conrad L. Wirth, director, National Park Service (also in the area of conservation and resources).

These awards, which will be presented at a luncheon in Washington in April, are made possible by a fund established by John D. Rockefeller III that is administered by the Woodrow Wilson School of Public and International Affairs at Princeton University.

Although the fund has been administered since 1952, this year the character of the awards has changed, for previously they were designed to make additional self-training possible for government employees in mid-career. With the passage of the Training Act of 1958, which gave broad training authority to all executive agencies of the government, a principal objective of the original awards was realized. Therefore the new Rockefeller Public Service Awards Program places primary emphasis on achievement and long and distinguished career service.

The $5000 Rumford Premium of the American Academy of Arts and Sciences was awarded on 8 March to **Charles H. Townes**, professor of physics at Columbia University, originator of the maser. (The word is an acronym deriving from *m*icrowave *a*mplification by *s*timulated *e*mission of *r*adiation.) The award recipient must be "the author of the most important discovery or useful improvement which shall be in any way made known to the public . . . on Heat or on Light; the preference always being given to such discoveries as shall . . . tend most to promote the good of mankind."

The maser, which is being further developed by a research team under Townes's direction, has enabled astronomers to determine such matters as the temperatures of Venus and Jupiter. In other fields it is used to measure time, to improve long-distance communications, to check Einstein's special theory of relativity, and as a tool in molecular and electronic research.

In 1960 Townes and A. L. Schawlow of the Bell Telephone Laboratories announced the development of an optical maser which is expected to have great importance in the field of space communications.

I. I. Rabi, professor of physics at Columbia University, was honored by the Society for International Scientific Relations at a dinner meeting on 28 February at Columbia's Faculty Club. On behalf of the society, Detlev W. Bronk, president of the National Academy of Sciences, presented a medal to Rabi for his outstanding contributions to science and society. Rabi delivered an address on "Science and Public Policy."

Sydney Chapman of the High Altitude Observatory, Boulder, Colo., will discuss "Sun Storms and the Earth" as a Sigma Xi national lecturer at a number of colleges and universities during April.

Marvin L. Goldberger, professor of physics at Princeton University, has been awarded the Heineman Prize for outstanding achievement in mathematical physics. He will receive the $2500 prize on 26 April at the annual dinner of the American Physical Society, in Washington. The award is endowed by the Heineman Foundation and administered by the American Institute of Physics. A committee appointed by the American Physical Society selects the winner.

Sherwood L. Washburn, professor of anthropology at the University of California, Berkeley, was accorded one of the highest honors in his field on 3 March, when he was awarded the Wenner-Gren Foundation's 1960 Viking Fund Medal and $1000 prize. Washburn, who is an expert on baboon behavior, was nominated for the award by the American Association of Physical Anthropologists.

Pierre L. Balligand will succeed **Hubert de Laboulaye** as deputy director general in charge of the department of technical operations at the International Atomic Energy Agency, Vienna. Balligand is the head of the "service des grandes piles experimentales" (the division of large research reactors) of the French Commissariat à l'Energie Atomique. De Laboulaye will leave the IAEA toward the end of March to return to a senior position with the French Commissariat.

Herbert Friedmann, head curator of zoology at the U.S. National Museum, Washington, D.C., has been named director of the Los Angeles County Museum in Exposition Park, Los Angeles, effective 1 April.

Recent Deaths

Floyd E. Bartell, Ann Arbor, Mich.; 77; professor emeritus of chemistry at the University of Michigan and a colloidal chemist; on the faculty for 43 years before retiring in 1953; 5 Mar.

Johannes H. Bauer, Baltimore, Md.; 71; internationally known research physician; a pioneer in developing a vaccine for yellow fever, he did most of his work through the Rockefeller Institute, was in charge of the laboratories of the International Health Division of the Rockefeller Foundation in New York; 4 Mar.

Enzo Beri, Italy; professor and chairman of the department of human physiology, Medical School, University of Ferrara, Italy; director of Group 3-C of the Biology Division of the Italian Nuclear Research Center; former Rockefeller fellow at the Nobel Institute for Medical Research in Stockholm, under H. Theorell, and a former research associate at the Johnson Foundation of the University of Pennsylvania, under Britton Chance.

Maurice B. Linford, Urbana, Ill.; 59; professor of plant pathology, University of Illinois; formerly head of the plant pathology department, Pineapple Research Institute of Hawaii, where he did notable work in plant nematology; 24 Sept.

Lord Stopford (John Sebastian Bach Stopford), Arnside, Westmoreland, England; 72; anatomist, who was vice chancellor of Manchester University from 1934 to 1956; was one of the first 14 persons to be granted life peerage, in 1958; professor of anatomy at Manchester (from 1919 to 1937) and dean of its medical school; wrote *Sensation and the Sensory Pathway*, in 1930, and many papers on anatomy, education, and neurology; had held many important posts, including the chairmanship of the Universities Bureau of the British Empire; 6 Mar.

Book Reviews

Palaeolithic Art. Paolo Graziosi. McGraw-Hill, New York, 1960. 278 pp. Illus. + plates and maps. $35.

Because this review is written for *Science* and not for *Art*, it will cover principally the scientific facts and fancies which can be deduced from an exhaustive, beautifully illustrated, and comprehensive volume on European Upper Paleolithic art, and the volume is cheap at the price. This art arose, from no known preceding school, in southern France and northern Spain, at or near the beginning of the Upper Paleolithic period, some 30,000 years ago; and it achieved a vigor of realistic depiction, in several media, almost at the start, and culminated in the Late Magdalenian polychrome paintings in two caves, Altamira in Spain and Font-de-Gaume in France. The former is well preserved, but in the latter the paintings have been nearly destroyed by 50 years of visitors rubbing the paintings with fingers and overcoats, breathing on them, and carving their names on them with penknives. In one cave, Rouffignac, discovered only in 1956, a criminal named Dubois has managed to hack his surname over the painting of an extinct rhinoceros.

In nearly all these caves what is now a bent but magnificent old man, without whose tireless efforts and artistic skill we might know much less about this art, crawled for decades, tracing and copying, and elsewhere he later analyzed what he had carried away. He is the Abbé Henri Breuil, whose name, as it should, occupies a third of a column in the index, and to whom I pay respectful tribute.

Graziosi divides Upper Paleolithic art into two categories: portable (he calls it mobiliary, an awkward translation unsanctioned by Webster) and stationary. Portable art consists of carvings, etchings, and a few paintings, done on antler, ivory, and various stones, all of which a cave man could carry about with him. Stationary art is what he etched, carved, and painted on the walls of caves and rock shelters, as well as a few precious clay models of animals found leaning against walls in the innermost and most secret recesses of a very few caves.

Portable art has been found in 110 sites, distributed as follows: France, 58; Spain, 10; Germany, 10; Italy, 9; U.S.S.R., 8; Belgium, 5; Czechoslovakia, 4; Switzerland, 2; England, Austria, Hungary, and Poland, 1 each. These sites are concentrated in the following regions: southwestern France, between the Dordogne and Loire rivers; the northern flanks of the Pyrenees around the sources of the Garonne River; the Basque coast of Spain, near Santander and Bilbao; and other concentrations long the Rhone, Rhine, upper Danube, Dnieper, and Don.

Stationary art is much more limited; France has 65 sites, Spain 29, and Italy 3. The three main centers are along both banks of the Dordogne River; in the French Pyrenees, as above; and in the Spanish Basque region. Secondary clusters are found in the Rhone Valley and in the distal tips of Spain and Italy.

The European art world of the last part of the Pleistocene is divided by Graziosi into three principal provinces: the Franco-Cantabrian, which is the central one; the eastern European; with an excursion into Siberia as far as Lake Baikal; and the Mediterranean. Portable art is found in all three, while stationary cave art is concentrated in the first and is found less extensively in the third.

The eastern European province extends as far west as Predmost in Czechoslovakia. Its art consists mostly of ivory sculptures, which are so conventionalized that we are not sure whether they represent phalli, women, or birds. It also includes ivory plaques which could have been worn as bracelets or wrist guards, and a decorated mammoth's jaw and scapula. All this art is geometric. Vestonice, also in Czechoslovakia, is the eastern frontier of the Franco-Cantabrian art.

The art of the Mediterranean province has both portable pieces and cave engravings and paintings. While in general it is abstract, some of its human representations are the most realistic of all. In what follows it will be lumped with the Franco-Cantabrian product.

The materials of portable art are ivory, antler, bone, limestone slabs, steatite, stream pebbles, and clay. Those of stationary art are the cave walls themselves, cave clay, and pigments of magnesium, red and yellow ochre, and kaolin. The tools used on both were burins of several kinds and sizes, possibly tubes for spraying pigment in solution, and probably spatulas. Lamps have been found in which animal fats were probably burned. How the artists reached the ceilings is unknown.

The time is from the Lower Perigordian through the Aurignacian, Solutrean, and Magdalenian, a span of nearly 20,000 years. Portable art goes through without a halt, but the cave painting is believed to have stopped temporarily during the Solutrean, which was a time devoted to sculpture in the near-round on the walls of rock shelters and at the mouths of caves. It is very hard to date cave paintings in any case, and much of the chronology rests on estimates of style.

Style and Subject

Nevertheless there is continuity in art style from beginning to end, which suggests a continuity in people and in their cultural traditions. The succession of flint knapping techniques, upon which archeologists relied for their chronology before carbon-14, can now be taken as a succession of technical inventions or diffusions, or both, without necessary change of personnel, for otherwise the art would have died or changed.

The subjects of this art are mostly animals, the depictions of which are as useful to zoologists as to esthetes. Mammoths have high domed heads, shaggy hair, and, as one would expect in a cold climate, small ears. Bison resemble the surviving European species. In *Bos primigenius* the bull has a black coat and the cow a red one, which is correct. Horses look like tarpans, although in the Mediterranean province, at least, some suggest *Hemippus*, the half-ass, recently extinct in western Asia. The deer family is represented by elk

(moose), red deer (elk), reindeer, and fallow deer; the pig tribe by *Sus scrofa*. The ibex is common, as are bears both cave and brown, and felines hard to identify further. One curious beast in Lascaux might be *Pantholops* or *Oryx*. Fish are well drawn, representing salmon, pike, and sole, but birds are drawn sketchily, like people; the vegetable world is hardly shown at all.

Human subjects are mostly exaggerated or caricatured. Only in two sites, La Magdaleine in France and Addaura in Sicily, are realistic cave engravings of normal people to be found. In statuettes, cave entrance bas-reliefs, and engravings on loose rocks, the usual human subject is an obese woman similar to some seen in diabetic clinics today, and their kind of obesity is familiarly Caucasoid. In some the external genitalia are emphasized in a manner frowned on by the U.S. Post Office. Wall engravings and paintings show facial features, beards, hair, and even skin color which are as European as those of the living inhabitants of Franco-Cantabria. The realistically lounging bodies of two women in La Magdaleine and the figures of several men in Addaura are the same. These representations assure us that the soft parts of the Upper Paleolithic Europeans of that region were as Caucasoid as their skulls and long bones and, thus, should dispel the well-publicized fancied resemblances between certain Upper Paleolithic skulls and those of Eskimos and other non-Caucasoid peoples.

Beside animals and men, the sculptures, engravings, and paintings represent a number of unidentified objects; tectiforms, shield signs, blazons, club signs, and projectile signs. Tectiforms are line drawings, pentagonal in section, which look like diagrams of huts with overhanging eaves and central poles. Sometimes human or animal figures rest in or on them, sometimes other lines crisscross them, as if to represent poles or cords. These representations have been called huts, tents, tread traps, and even spirit-placings. No one, of course, really knows what they were.

Shield signs look exactly like oval shields. Blazons are rectangles irregularly divided into six or more smaller rectangles of irregular disposition which are painted different colors, and looking for all the world like the art of Mondrian. These have been called tribal coats of arms. In Lascaux an ungulate has its front hoofs resting on two of these, and this juxtaposition could make them shaky pit covers, or be just a coincidence. Club signs look like hafted axes or knobbed maces. Certain shafts feathered at the butt and often sticking out of animals, or even the buttocks of one man, probably represent darts cast in spear throwers, of which many have been found, rather than arrows, for we have no evidence of the use of the bow except possibly in Spain.

In the portable art, particularly engravings on antler and ivory, attempts were made to depict scenes involving several animals, or even herds. In a carved spear thrower head, two ibexes face each other with forelegs locked, apparently wrestling. But in the cave art almost all animals are presented without reference to other compositions; in fact, they are often superimposed. This disregard for earlier work reminds me of the practice of Australian aboriginal cave artists today; periodically they visit sacred rock shelters to renew old paintings, either by retouching or by painting over them. The motive of these artists is apparently a desire to renew and to intensify old relationships between men and the timeless spirit world, the same one which impels these very aborigines to act out totemic rituals. The result, of course, is a reenforcement of human relations within the group concerned.

Interpretations

Many archeologists, ill-versed in ethnology and ignorant of the functional point of view, have conjured up fan-

Deer, shown with few details but perfectly executed relief, from the cave of Niaux, France. The painting is in a circular chamber 800 meters from the opening of the cave. [From *Palaeolithic Art*]

tastic reconstructions of what went on in these caves, emphasizing particularly sexual orgies and the initiation of frightened youths. Fat women and pregnant or copulating animals are read as fertility symbols, but primitive hunters know little of fertility, even today. Their job is to catch, not to breed, animals, and as every hunter knows an animal is most easily killed when preoccupied with sex or heavy with young. Fat women are no more fertile than skinny ones, just softer, warmer, and more conspicuously well-fed.

Another cliché of cave-art interpretation is that the masked or disguised men are all potent shamans or sorcerers. In primitive societies masks and disguises are almost universal and serve several purposes. Many of them, particularly among hunters, represent animals. They may symbolize an animal who once entered into a close relationship with an ancestor through some kind deed. Wholly without abstract symbolism they may represent a hunter disguised as an animal and mingling with an unsuspecting herd until close enough to strike.

Such disguises can be used in ceremonies, even little plays, which welcome the ancestral spirits home for the winter and bid them good-bye in the spring, or as disguises to conceal the identities of individuals in initiations or secret executions. If these figures represented deities such as the "owners" of animal species, one might expect them to be executed with as much care as the animal figures themselves, which is rarely the case.

In the Addaura cave etchings an actual scene is shown. Two men, apparently trussed up Commando-style, with heels held behind buttocks and tied by a cord running up their backs to their necks, flounder around with either erections or penis-sheaths in the midst of a circle of spectators whose arms are raised. What this window on the past means many have fancied but no one knows.

May I conclude by saying that looking at the plates of this book is an experience which cannot be conveyed by a subesthetic scientist in a review. Those interested will have to look at it themselves, or better yet visit the caves, with their hands behind their backs.

CARLETON S. COON
University Museum,
University of Pennsylvania

Special Relativity. W. Rindler. Oliver and Boyd, Edinburgh; Interscience, New York, 1960. 196 pp. Illus. $2.25.

This textbook of the special theory of relativity covers all the essentials of the subject at a level that can be managed by a better-than-average undergraduate. Most of the results are presented in both four-dimensional and three-dimensional notation. Many of the exercises present real challenges and cannot be answered without some conceptual effort. Though the book appears to be conceived primarily for the type of instruction common in British universities, it might well be used in the United States, either as a basic text in a regular course on special relativity, or in an undergraduate honors course or seminar.

PETER G. BERGMANN
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Syracuse University

The Bacteria. vol. 1, *Structure*. Irwin Clyde Gunsalus and R. Y. Stanier, Eds. Academic Press, New York, 1960. 513 pp. Illus. $13.

Not since Duclaux's *Traité de Microbiologie*, published more than half a century ago, has so comprehensive a treatise on a bacteriological subject been published as this series will represent. That five volumes are taken to do justice to the bacteria and that, even then, they deal almost exclusively with the true bacteria testifies to the amazing expansion of bacteriology, especially in the last 20 years.

Whereas the undertaking was heroic, the fulfillment, judging from the initial volume of the series, has to be regarded as short of outstanding. True, most of the important, recent, factual information on eubacterial anatomy is there; one can find no major fault with the adequacy of the volume in this regard. Yet, in spite of this and in spite of the major-league caliber of the contributors, this particular book is unlikely to become the microbiological classic to which it obviously aspires.

To those familiar with Topley and Wilson's masterly *Principles of Bacteriology and Immunology*, a treatise which in the coverage of its particular areas is comparable to *The Bacteria*, the contrast between the two major works is immediately evident. If I were allowed but one observation to distinguish the two, I would say that Topley and Wilson synthesize their field whereas Gunsalus and Stanier dissect theirs. Both stylistic approaches have their devotees, but the influences and longevity of the former are greater and infinitely more stimulating and gratifying.

This first volume of the proposed set is a tremendously uneven collection of reviews, divided according to the various anatomical parts of the bacterial cell. To be sure, the editors apologize in the preface for the disparities, but even they must have—or should have—grimaced painfully when sandwiching these chapters between the covers of one book. The conspicuous, avoidable defects of the last comparable multi-authored treatise on bacteriology—namely, Werkman and Wilson's *Bacterial Physiology*—should have forearmed Gunsalus and Stanier. This raises the question of the function of the editors in an enterprise of this nature, on the assumption that they are not mere figureheads.

In short, this volume, while doing a competent journeyman job, is singularly imperceptive of the larger and more meaningful opportunity which still cries for materialization. The void is all the more conspicuous now, but we shall be sustained by the belief that some microbiological scholar, who is also facile of expression, will, with perspective and insight, fuse the diverse elements of bacteriology into a profluent magnum opus that will be a focal landmark, as well as a milestone, in the field.

The individual chapters are as authoritative as one could wish; they were written by investigators most of whom are regarded by some others, and by themselves, as high priests of bacteriology. The articles are virtual ringers for the several reviews on the same topics which have appeared in various journals and symposium volumes during the past few years under the names of many of the very same authors. As mentioned above, this is essentially a collection of updated reviews. Beginning students and other novices will be greatly serviced by this convenient source of salient facts and by the extensive up-to-date bibliographies. The book, and its succeeding companion volumes, will be a godsend to every Ph.D. candidate in all branches of microbiology.

Disregarding the heterogeneity and disproportion of the treatments, most of

the chapters are, it seems to me, overly drawn out for a work of this nature. The reading is frequently tedious, minor items are belabored, the messages diffuse. It is a pity that the editors didn't impart more muscle to the book by insisting on a substantial condensation of several of the chapters. Lest the argument be raised that the breadth and scope of certain chapters are justification for prolixity and trivia, I refer to T. F. Anderson's superb exposition on bacterial viruses. This is a model of incisiveness and of the presentation of basic facts and general principles on a subject which is broader in scope and diversity than any of the other subjects in the book. I also thought that Lennox on immunological analysis and Weibull on locomotion have the elements of a suitable pedagogical complexion.

The individual author system, as employed here, reveals the need for wise editing to curtail repetitious and overlapping treatments and to insure inclusion of material borderline to the assigned topics; it would also mean exercising firm control over space apportionment and style, and ruthless pruning. I imagine this situation to be little different from a conductor's responsibility for forcing a balance among the musicians in his orchestra, even the virtuosos. Reiteration and overlapping are particularly noticeable and irksome in connection with information on the chemical composition and on the properties and functions of the bacterial cell wall. Also, since the protoplast and the cell wall are intimately bound up with each other in the living cell, the artificial separation of the treatments of the two is not only conducive to redundancy, but is unrealistic and unnatural.

On the other side of the ledger, if one is willing to condone the exclusion of the remarkable body of knowledge which exists on yeast cytology and of a consideration of actinomycetes, myxobacteria, and perhaps other investigations on less studied bacteria, one will find in the book practically all of the significant, basic information available at the deadline for receipt of manuscripts. There can be no doubt about the timeliness and the value of this volume for anyone seeking intimacy with bacteria.

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Bionomics, Systematics, and Phylogeny of Lytta, a Genus of Blister Beetles (Coleoptera, Meloidae). Richard B. Selander. University of Illinois Press, Urbana, 1960. vi + 295 pp. Illus. $5.50.

The main portion of this paper is a monograph of the North American species of *Lytta*. The 69 species recognized are classified in four subgenera, 13 species groups, and 11 subgroups; each category is defined and characterized, often partly on the basis of bionomic information. There is an artificial key to species; there is also another set (more difficult for the beginner to use) designed to show relationships, with separate keys for each subgenus, species group, and subgroup.

The introductory portion contains a summary of the bionomics of the genus on a world basis, a redefinition of the genus, and a reclassification which places *Poreospasta* and *Pomphopoea*, previously recognized as separate Nearctic genera, as subgenera, and which adds five new subgenera, two of which are North American. The classification that results is certainly more satisfactory than its predecessors and is the first that really interrelates the world fauna.

For the treatment of the North American species, Richard Selander has examined a large portion of the specimens in collections and has carried out extensive field work himself, particularly in Mexico. Some of the species of *Lytta* are not easy to obtain for study. They may be very abundant in a limited area at one time and then not be seen again for many years. Some species have been collected only once, even large and strikingly marked species that would attract the attention of any entomologist. The extreme fluctuations in population are puzzling, but no more so than the sources of the enormous numbers of individuals of large species sometimes produced. The larvae of all species for which the life history is known live in the nests of solitary bees, feeding on stored pollen and probably on immature bees. At times a greater mass of *Lytta* protoplasm seems to have been produced than can be accounted for by the numbers of bees in the area.

At the species level the classification is a conservative one. Geographic variation, involving both anatomic and color pattern characteristics, has been analyzed in detail for polymorphic species. The author has chosen to use vernacular names for geographic races that are very distinct, as suggested by Wilson and Brown. Names that we might expect to be retained as Latinized subspecific names have all been synonymized, including two previously proposed by Selander himself. The classification benefits; it is simple and easily followed.

The diagnostic features of each species have been illustrated adequately, and the ranges have been indicated by small maps in the text. I am happy to see individual localities shown; this is a particularly valuable feature because one often encounters difficulty in identifying some of the older localities in a list of records. A considerable amount of geographic detective work must have been necessary to produce these maps, particularly those for Mexico and California, two regions where many collectors have used almost meaningless names for localities all too frequently. The text ends with a detailed index, a rare item in a systematic paper.

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The Luapula Peoples of Northern Rhodesia. Custom and history in tribal politics. Ian Cunnison. Manchester University Press, Manchester; Humanities Press, New York, 1960. xiv + 258 pp. Illus. $6.50.

The African peoples described in this book live on either side of the lower Luapula River, the boundary between northeastern Rhodesia and the Congo. They are principally fishermen, for the environs of the river are fertile swamps. The peoples are of particular interest, because, although on the Rhodesian side, they comprise a single society under a paramount chief, they are culturally heterogeneous and are of several tribal origins. This book is a study of political and social integration occurring in a situation of persistent cultural diversity. Cunnison found himself compelled to begin his analysis from the point of view of Luapula histories. These people have a marked interest in their oral traditions; although Cunnison recognizes that the oral traditions are not necessarily correct accounts of the past, he admits their validity for the

peoples themselves, for they use them as codes of precedent for status, rights, and cultural activities. The oral histories are more than this, however, for they contain sociological explanations of the contemporary society; the nature of these explanations enables anthropologists to find the best approach for examining key institutions and the state of unity within diversity.

The focal group, the Lunda people, is designated and accepted by all Luapula peoples as the rulers; among the Lunda are found not only the paramount chief but also many other persons with official authority and privilege. Although this has been a conquest-state for more than a century and a half, Lunda cultural influence tends to be limited to a number of political centers, and little effort has been made to establish conformity. The Lunda chiefship and political authority is, nevertheless, linked directly and indirectly to the institutions of the various peoples; it provides a coherent structure and a stable continuity to the society. This book provides a useful addition to our knowledge of central African peoples.

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Implications of Evolution. G. A. Kerkut. Pergamon Press, New York, 1960. x + 174 pp. Illus. $5.

"It may be distressing for some readers to discover that so much in zoology is open to doubt, but this in effect indicates the vast amount of work that remains to be done." Kerkut made this discovery for himself, and this book is his cry of distress. Evolution has not, he writes, been proved beyond all reasonable doubt. He is not an antievolutionist, since he concedes "that many living animals can be observed over the course of time to undergo changes so that new species are formed." But he argues, correctly of course, that we do not know exactly when and how life arose from nonliving matter (chapter 2), that the phylogenetic relationships of the viruses, rickettsiae, and bacteria are obscure (chapter 3), ditto for the protozoans (chapter 4), ditto for the origin of the Metazoa (chapters 5 and 6), ditto for the invertebrate phyla (chapter 7), ditto for the origins of the vertebrates as suggested by the biochemical studies (chapter 8). All of which means that zoology is not yet a completed story, and, indeed, a vast amount of work remains to be done.

The basic conclusion of the author is, however, something else—since we cannot yet reconstruct in all details the phylogeny of the animal kingdom, therefore, evolution is not "proven"! This is a confusion of two distinct problems; we may be sure that life (or, for that matter, the Cosmos) had a history, but it does not follow that we know all the events of which these histories are composed, with their respective dates. The author has been wise not to suggest any alternatives to the theory of evolution; he has been unwise to write chapter 9 ("Vertebrate palaeontology") without which the book would have been better than it is.

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New Books

Biological Sciences

Biological Clocks. vol. 25 of *Cold Spring Harbor Symposia on Quantitative Biology*. Biological Laboratory, Cold Spring Harbor, N.Y., 1960. 524 pp. Fifty papers presented at the symposium which was attended by 150 scientists from 31 countries.

The Biological Role of Ribonucleic Acids. Jean Brachet. Elsevier, Amsterdam; Van Nostrand, Princeton, N.J., 1960. 144 pp. The 1959 Weizmann Memorial Lecture.

Dictionary of the Fungi. G. C. Ainsworth. Commonwealth Mycological Inst., Kew, Surrey, England, ed. 5, 1961. 554 pp. Illus. 30s. More than 5000 corrections or alterations have been made, new material added, and new illustrations prepared; 500 more generic names have been compiled—mostly from vol. 2, pts. 8–20, of the Institute's *Index of Fungi*.

Electrophysiological Methods in Biological Research. Josef Holubar and Josef Ipser, Eds. Translated by Petr Hahn. Czechoslovak Acad. of Sciences, Prague, 1960. 515 pp. Illus.

Evaluation of Drug Therapy. Francis M. Foster, Ed. Univ. of Wisconsin Press, Madison, 1961. 191 pp. $4. Proceedings of a symposium held in May 1960.

Index-Handbook of Cardiovascular Agents. vol. 2, pts. 1 and 2 (1951–1955). Publ. No. 821. Isaac D. Welt, Director, Literature Project. National Acad. of Sciences–National Research Council, Washington 25, 1960. 1623 pp. $15. Lists 13,400 scientific communications, each analyzed and annotated with reference to the various drugs involved. Chauncey D. Leake served as chairman of the advisory committee; the staff included Welt, James H. Defandorf, Carter Lee, Judith MacMillan, and Edgar C. Rich.

Mackie and McCartney's Handbook of Bacteriology. A guide to the laboratory diagnosis and control of infection. Robert Cruickshank, Ed. Livingston, Edinburgh; Williams and Wilkins, Baltimore, Md., 1960. 991 pp. $8.50.

Die Neuroendokrine Steuerung der Adaptationstatigkeit. K. Lissak and E. Endroczi. Akademie der Wissenschafter, Budapest, 1960. 171 pp. Illus.

Orthopedic Surgery of the Dog and Cat. Ellis P. Leonard, Saunders, Philadelphia, Pa., 1960. 308 pp. Illus. $12.50.

Principles of Plant Breeding. R. W. Allard. Wiley, New York, 1960. 496 pp. Illus. $9.

General

An Atlas of Africa. J. F. Horrabin. Praeger, New York, 1960. 126 pp. Cloth, $3.50; paper, $1.45. Graphic maps showing the geography, topography, and history of Africa. A section entitled "Tomorrow" shows problems of population, language, and transportation; distribution of mineral wealth, water, and water power; recent activities in the Sahara; and sites of the main archeological activities in recent times. The volume measures approximately 5½ by 8 inches.

Cacti. Walter Kupper and Pia Roshardt. Translated and edited by Vera Higgins. Nelson, New York, 1960. 127 pp. Illus. $10. A description of the spines, flowers and fruits; written for the nonscientists. The 60 full-page illustrations (in color) by Pia Roshardt were painted from specimens selected from the Cactus House in Zurich.

God of the Scientists. God of the Experiment. Remy Chauvin. Translated by Salvator Attanasio. Helicon Press, Baltimore, Md., 1960. 159 pp. $3.95.

Government Publications and Their Use. Laurence F. Schmeckebier and Roy B. Eastin. Brookings Institution, Washington, D.C., ed. 2, 1961. 487 pp. $6. A description and evaluation of catalogs, indexes, bibliographies, and other sources of information; explanation of classification and titling systems, with new chapters on government periodicals and microfacsimile copies of government publications.

Klimadiagramm-Weltatlas. vol. 1. Heinrich Walter and Helmut Lieth. Fischer, Jena, East Germany, 1960. The first part (three volumes, with about 30 maps and their supplements, are planned) of a collection designed to represent the climate of the world in uniform graphic reproduction, each map showing climate regions and position of meteorological stations. Supplementary charts provide information about stations, altitude, years of observation, mean annual precipitation, and so forth. The authors are staff members of the Institut der Landwirtschaftlichen Hochschule.

The Mediterranean Lands. D. S. Walker. Methuen, London; Wiley, New York, 1960. 547 pp. Illus. $6.75. Primarily intended for the sixth form in British schools, this book deals in some measure with all the countries possessing a coast line on the Inland Sea.

Man and Space. The next decade. Ralph E. Lapp. Harper, New York, 1961. 190 pp. $4.95.

Reports

Natural Occurrence of Malaria in Rhesus Monkeys

Abstract. A relatively high incidence of malarial infections has been noted in recent importations of rhesus monkeys from Pakistan. This finding, contraverting the widely held belief that rhesus monkeys are free of natively acquired malaria, is of substantial significance to those employing Pakistanian rhesus monkeys in their investigations.

This laboratory has pursued studies on the biology and therapy of primate malarias since 1945. Between this date and July 1960, some 5600 rhesus monkeys (*Macaca mulatta*) have been used in these investigations without evidence of the natural occurrence of malaria in any subject. In July 1960, one animal imported in December 1959 was found to be infected. With unwavering conviction that rhesus monkeys are not natively infected, we attributed the disease in this subject to an undefined "laboratory accident." Events of the past 2 months have made it necessary to reevaluate this conclusion.

On 19 October 1960, 100 monkeys recently imported from Pakistan were introduced into the colony for conditioning purposes. In January 1961, prior to initiation of a malaria experiment, thick blood films were prepared on 79 of these animals, stained with Giemsa, and examined for the presence of blood parasites—a routine practice despite years of negative results. To our amazement, seven of these stock animals had positive thick films. This finding led immediately to examination of the animals of three later shipments, also imported from Pakistan. This resulted in findings of positive films for 13 of 65 animals imported on 23 November 1960, for one of 55 of a 15 December shipment, and for one of 29 in a 22 December shipment. In all, 22 of 228 monkeys from these four shipments were found to be naturally infected. The density of infection ranged from one to two parasites per entire thick film to 2×10^7 parasites per milliliter of blood. In the majority of subjects a single plasmodium appeared to be involved. In two cases there were evidences of multiple infections.

Identification of the plasmodial species is still in progress. Data acquired to date from thin-film studies, both on monkeys with naturally acquired infections and on others in whom disease was induced through inoculation of infected blood, suggest that the parasite found in the majority of cases is the morphologic counterpart of our stock strains of *Plasmodium inui*. In the two cases suggestive of multiple infections, a plasmodium morphologically similar to *P. cynomolgi* also appears to be involved. In both natural and induced infections the disease appears to follow a rather benign course. In the induced disease the parasitemia is intensified by splenectomy.

The observations reported here (*1*) are at variance with the widely held belief that the rhesus monkey is free of malaria in its native habitat. This belief cannot be dismissed lightly, since it is supported by some 30 years of careful study by investigators who have employed the rhesus monkey in work on the biology and therapy of simian malarias. It appears likely to us that the conflict between findings in earlier investigations, in which we participated, and the current observations is related to the area from which the animals were obtained. For many years the great majority of rhesus monkeys have come from areas adjacent to Lucknow. Monkeys in recent shipments from this region, like those of shipments in former years, have been free of malaria. On the other hand, the infected animals originated in East Pakistan, having been exported from Dacca. Although there is no absolute certainty in this matter, it is possible that they were obtained near the Burma-Assam border. If this is the case, the animals could have been in contact with *Macaca cynomolgus* or *M. nemestrina*, simian subjects frequently infected with *Plasmodium knowlesi*, *P. cynomolgi*, or *P. inui*. It is not too difficult to believe that the same vectors which transmit these plasmodia to the crab-eating and pig-tailed macaques could also transmit them to the rhesus monkey. There is evidence in the old literature on malaria that such an event does occur. Thus, in 1911, Mathis and Leger (*2*) found quartan infections (probably *P. inui*) in five of 40 rhesus monkeys collected in Tonkin. *Plasmodium inui* infections of *M. cynomolgus* are relatively common in this area.

The importance of these observations on the natural occurrence of malaria in Pakistanian rhesus monkeys to investigators of plasmodial diseases needs no comment. It is equally apparent that workers employing rhesus monkeys in other investigations should be alert to the existence and potential liabilities of concurrent malarial infections.

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Reference and Note

1. This study was supported in part by grant No. E-2372 from the National Institute of Allergy and Infectious Diseases, Bethesda, Md.
2. C. Mathis and M. Leger, *Ann. inst. Pasteur* **25**, 593 (1911).

16 February 1961

Stimulus Generalization of Conditioned Suppression

Abstract. A tone ending in unavoidable electric shock was periodically presented to pigeons while they pecked a key for food. When pecking was completely disrupted by tone, shock was disconnected, and the training tone and tones having new frequencies were presented. Initially the gradient of generalization was broad; as testing proceeded, however, the gradient narrowed severely.

When a stimulus has typically preceded a noxious event, subsequent presentation of that stimulus will often cause a reduction in the frequency of ongoing responses. This reduction is referred to as conditioned suppression. In the present research we sought to investigate the degree to which conditioned suppression could be mediated by stimuli which were like, but not identical to, the stimulus that was used in the original training. This latter phenomenon, the stimulus generalization of conditioned suppression, is of particular importance, for it represents

Instructions for preparing reports. Begin the report with an abstract of from 45 to 55 words. The abstract should *not* repeat phrases employed in the title. It should work with the title to give the reader a summary of the results presented in the report proper.

Type manuscripts double-spaced and submit one ribbon copy and one carbon copy.

Limit the report proper to the equivalent of 1200 words. This space includes that occupied by illustrative material as well as by the references and notes.

Limit illustrative material to *one* 2-column figure (that is, a figure whose width equals two columns of text) or to *one* 2-column table or to *two* 1-column illustrations, which may consist of two figures or two tables or one of each.

For further details see "Suggestions to Contributors" [*Science* **125**, 16 (1957)].

a process by which the effects of aversive training may come to encompass large segments of an organism's behavior (*1*).

Two carneaux pigeons, deprived to 80 percent of their free-feeding body weight, were taught to peck a key in the wall of the experimental chamber. This behavior was maintained by occasionally reinforcing a key peck with food (VI-2). The noxious event was electrical shock which was administered to the birds by way of a pair of permanently fastened wing bands. A coiled wire connector was clipped onto the wing bands, and the other end was fastened to a shock power terminal at the top rear of the experimental chamber. After initial training to peck the key, the birds were always run with the connector in place. With the occurrence of high stable key-pecking rates, the tone which was to be used as the suppressing stimulus and the tones to be used in assessing stimulus generalization were presented without shock. This operation permitted the effects of novel stimulation to adapt out. The tones each lasted 48 seconds and were equated for intensity (80 db spl). There were seven tones selected such that their frequencies were equally spaced along a logarithmic scale. When the bird's rate of key pecking was indifferent to the presence of any of the tones, suppression training was initiated. During suppression training only the middle frequency in the sequence was presented (1000 cy/sec), and unavoidable shock was programmed during its final 8 seconds. The tone-shock combinations were presented at irregular intervals. Since the programming of the variable interval schedule of food reinforcement was independent of that of the tone-shock pairings, food reinforcement could occur at any time during the tone and during shock. Training was continued until the key-pecking rate during the initial 40 seconds of tone was minimal and the rate during nontone periods was normal.

With the achievement of stable suppression, the use of shock was permanently discontinued, and without altering the schedule of food reinforcement the tests for stimulus generalization were conducted. The test sessions were run daily and lasted 2 hours. During a given test session each of the seven tones was presented once with the order of presentation randomized by means of Latin squares.

The magnitude of suppression during a tone may be estimated by comparing the number of responses made during the initial 40 seconds of the tone period (tone R's) to the number of responses made in the 40-second period prior to the tone onset (pre-tone R's). The relationship is expressed by the equation: suppression ratio =

$$\frac{\text{Pre-tone R's} - \text{Tone R's}}{\text{Pre-tone R's}}$$

The generalization gradients at several stages in the tests are shown for pigeons No. 5 and No. 9 in Fig. 1. Each gradient for No. 5 is the average for five sessions, while for No. 9 each gradient is averaged over four sessions. As can be seen in Fig. 1, generalization was at first broad, and as testing proceeded the gradients narrowed. In that no shock occurred during the tests, the changes in the shape of the gradient (the sharpening) during testing must reflect differences in the extinction of the suppressing capacity of the several stimuli. In general, the larger the difference in frequency between a given stimulus and that of the tone used in training (1000 cy/sec), the more rapidly did that stimulus lose its capacity to suppress ongoing behavior.

In the one previous study of the generalization of conditioned suppression (*2*), discrimination training was used to cause rats to suppress bar presses to an 1800-cy/sec tone but not to a 200-cy/sec tone. It was found that tones with frequencies that were intermediate between these extremes caused intermediate degrees of suppression. Since, however, the original training involved the acquisition of a discrimination, and

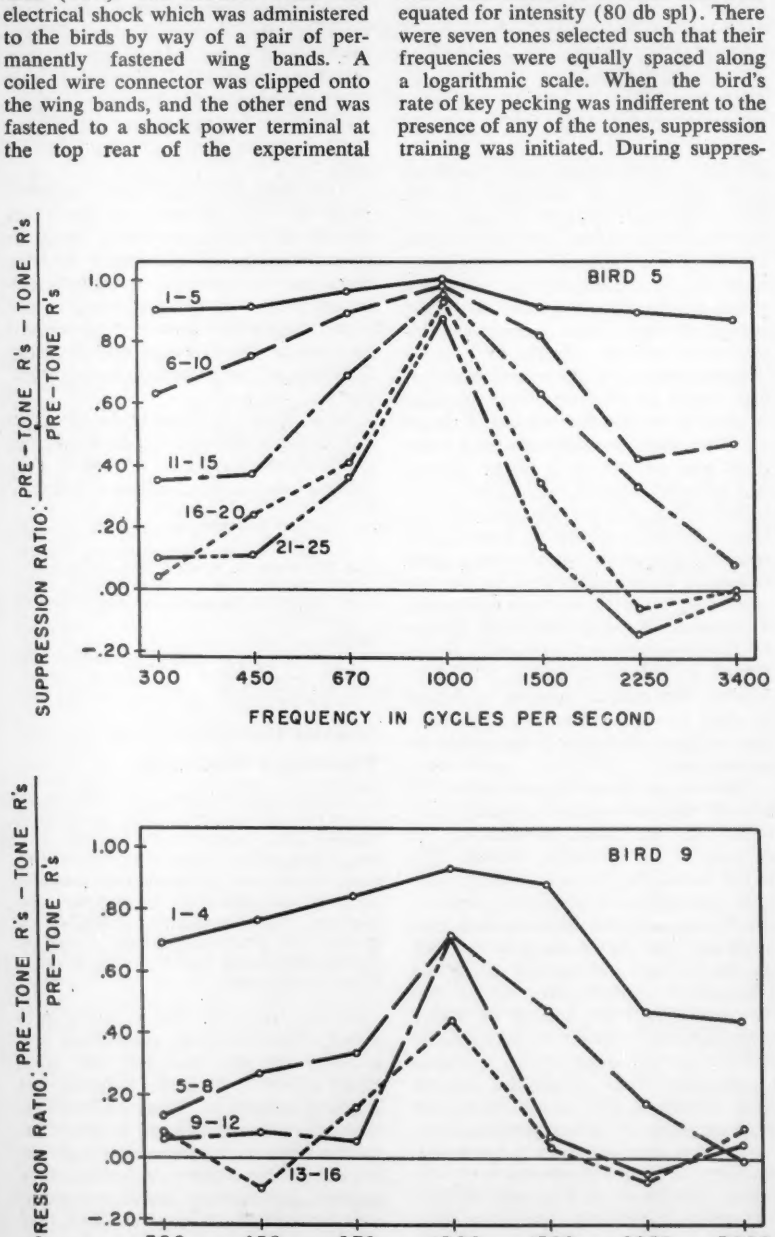

Fig. 1. Generalization gradients for birds No. 5 (top) and No. 9 (bottom). The numbers to the left of each gradient indicate the sessions included

since shock was used during the tests, the experiment provided no information about the nature of the gradient following training to a single stimulus; nor did it provide information about the changes in the shape of the gradient during the extinction of suppression.

The sharpening of the generalization gradient, during extinction, has also been reported in several studies involving the galvanic skin response (*3*), though in none were the effects as large as those herein described. It is perhaps of importance, however, that the methodology involved in the conditioned suppression paradigm and that involved in conditioning the galvanic skin response is similar. In both procedures, a noxious event is paired with a neutral stimulus.

Of the large number of other studies of stimulus generalization, only a few have reported data on changes in the gradient during extinction (*4*), and these indicate a variety of different results. At present the reasons for these differences are not clear (*5*).

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4. N. Guttman and H. I. Kalish, *J. Exptl. Psychol.* **51**, 79 (1956); H. M. Jenkins and R. H. Harrison, *ibid.* **59**, 246 (1960).
5. This research was supported by a U.S. Public Health Service grant (M-2433).

21 November 1960

Distribution of Strontium-90 in a 1959 Wheat Sample

Abstract. At least 22 percent of the strontium-90 found in a sample of wheat harvested in 1959 was due to direct deposition. Twenty-seven percent of the total strontium-90 content of this wheat sample was contained in the outermost bran layer.

Knowledge of the mechanism by which plants are contaminated by fallout is essential for the accurate prediction of future levels of Sr^{90} in foods. A particularly important fact that must be known is whether plants become contaminated principally from Sr^{90} that is absorbed from the soil or from Sr^{90} that results from the direct deposition of fallout on the plant surfaces. Studies presented here on the contamination of wheat may be of some help in resolving this problem.

Analyses, conducted by the Health and Safety Laboratory (*1*), of wheat and its milling products from 1958 and 1959 crops in the United States showed that the highest Sr^{90} concentration occurred in the bran fraction of wheat kernels. Similar results were reported by British investigators of the Agricultural Research Council Laboratory (*2*) for wheat grown in the United Kingdom and other countries in 1958.

The root uptake of Sr^{90} by wheat plants has been studied by C. C. Lee (*3*) in greenhouse experiments. He found that Sr^{90} absorbed from the soil is also concentrated in the bran.

Since the bran comprises the outer layers of the wheat kernel, it would be expected that much of the Sr^{90} found in this fraction is due to direct deposition. Lee's experiments indicate, however, that some of the bran contamination of field-grown wheat may result from root uptake.

Any directly deposited Sr^{90} that enters the wheat kernel must first pass through the outermost bran layer or epidermis. It is reasonable to expect, therefore, that a high proportion of the Sr^{90} in this layer is due to direct deposition. Experiments performed at the Health and Safety Laboratory on a sample of Ohio wheat harvested in 1958 showed that 25 percent of its Sr^{90} content was in the epidermis while this fraction constituted only 4 percent of the total weight of the wheat. To determine the origin of the Sr^{90} in the epidermal fraction of wheat kernels, further experiments measuring the ratio of Sr^{90} to stable strontium (specific activity) in the epidermis and in the remaining portion of wheat were conducted on a sample of Kansas wheat.

The separation of the epidermis was done by Theodore Earle (*4*) by a method, devised by him, which utilizes the friction produced in the controlled agitation of a mixture of grain and water to strip the epidermis from the wheat kernels.

Table 1 lists the results of analyses for Sr^{90}, stable strontium, and calcium performed on (i) a sample of hard red Kansas wheat, harvested in 1959, (ii) the epidermis stripped from the wheat, and (iii) the residual stripped wheat. The values tabulated are estimated to be accurate to within 10 percent.

Table 1. Distribution of strontium-90, stable strontium, and calcium in a sample of hard red Kansas wheat.

Percent of total	Strontium-90		Stable strontium		Calcium	
	$\mu\mu c$ in fraction	$\mu\mu c$/kg of fraction	mg in fraction	mg/kg of fraction	g in fraction	g/kg of fraction
			Original wheat kernel			
100.0	47.2	47.2	3.15	3.15	0.49	0.49
			Stripped wheat			
95.8	36.2	37.8	2.95	3.08	0.42	0.44
			Epidermis			
4.2	12.7	302.0	0.18	4.29	0.07	1.65
			Material balance			
	+1.7 (4%)		−0.02 (1%)		0.00 (0%)	

The material balances obtained indicate that the Sr^{90}, stable strontium, and calcium were not removed by the water used in the stripping operation.

Although strontium and calcium are chemically similar, they need not be physiologically identical. From data in Table 1, the ratio of stable strontium to calcium is found to be $0.18/0.07 = 2.6$ mg/g in the epidermis, while this ratio is $2.95/0.42 = 7.0$ mg/g in the stripped wheat. The difference in these ratios in indicative of dissimilar metabolic pathways for the soil-derived stable strontium and calcium. It is reasonable to conclude from these data that relatively little of the Sr^{90} found in the epidermis came from the soil.

Of the Sr^{90} found in the epidermis, that amount derived from root uptake may be estimated quantitatively by calculating the specific activity of the stripped wheat. If one assumes that the plant cannot distinguish stable strontium from Sr^{90} in the process of root absorption, the specific activity (Sr^{90} to stable strontium) of all parts of the wheat kernel should be the same. The specific activity of the stripped wheat is $36.2/2.95 = 12.3$ $\mu\mu c$ of Sr^{90} per milligram of stable strontium. This would be the specific activity of the epidermis, if there had been no direct deposition. The observed specific activity of the epidermis was $12.7/0.18 = 70.6$ $\mu\mu c$ of Sr^{90} per milligram of stable strontium; hence it appears that there must have been some direct deposition of Sr^{90}. The stable-strontium content of the epidermis was 0.18 mg; therefore, $12.3 \times 0.18 = 2.2$ $\mu\mu c$ of Sr^{90} must have resulted from root uptake. This is a maximum estimate since it has been assumed that all of the activity in the stripped wheat came from the

soil and none came by migration from the directly deposited Sr^{90} on the epidermis.

The epidermis contained 12.7 $\mu\mu$c of Sr^{90}. If 2.2 $\mu\mu$c of this total is a maximum estimate of the amount due to root uptake, then at least 10.5 $\mu\mu$c must have been due to direct deposition. The total Sr^{90} content of the entire wheat kernel was 47.2 $\mu\mu$c. Hence at least 22 percent of this total can be ascribed to direct deposition.

If the findings of this experiment can be generalized, a substantial drop in the Sr^{90} content of the 1960 wheat crop can be expected since the fallout rate and presumably the direct deposition of Sr^{90} on plants was much less in 1960 than in 1959 (*5*).

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1. "Fallout program quarterly summary report: December 1, 1959 through March 1, 1960," *U.S. Atomic Energy Comm. Rept. No. HASL-84* (April 1960), p. 100.
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26 October 1960

Preparation of a Floral Initiating Extract from Xanthium

Abstract. An extract from lyophilized tissue of flowering *Xanthium* (cocklebur) plants initiates floral development when applied to *Xanthium* test plants maintained on long-day photoperiodic conditions. Details of the preparation of the extract are described.

Hamner and Bonner (*1*) have submitted the first evidence of a hormone formed in *Xanthium* at the site of photoperiodic perception, the leaf, and later utilized by the bud in the transition from the vegetative to the flowering condition. They also record attempts to find a floral initiating preparation in extracts from flowering plant material. At the same time they tested numerous chemicals for activity. Although they did occasionally find evidence of floral-initiating activity in their plant extracts, they were unable to demonstrate any consistent pattern or to repeat their results with any confidence.

Table 1. Flowering response of *Xanthium* after application of extract.

Flowering response	Numerical av. of flowering response (*6*)
Untreated control plants	
All ten plants vegetative	0.0
Extract-treated plants	
Five plants vegetative (one plant, stage one; two plants, stage two; two plants, stage three)	1.1

Since that time there have been occasional published reports of preparations exhibiting floral-initiating properties. Such reports have not met the test of consistent repetition, with the exception of the now well-known effect of gibberellic acid in stimulating floral development in many long-day photoperiodically sensitive plants. Lang (*2*) suggests that gibberellic acid is probably not the primary floral-initiating stimulus, but that it acts indirectly in stimulating flowering. The action of gibberellic acid in promoting more rapid flowering in *Xanthium* (*3*) also appears to be an indirect effect.

The experiment described in the present report is the last of four successful demonstrations of floral initiation in *Xanthium* plants resulting from applications of as yet unknown substance(s) extracted from the branch parts of flowering plants. Our preparation has been independently checked by B. H. Carpenter and K. C. Hamner (*4*), and the results are in accord with our own.

Flowering branch tips were harvested from indigenous *Xanthium strumarium* L. var. *canadense* (Mill.) T. and G. (*5*) of the Long Beach area. The plants were picked when the staminate terminal inflorescence was from ½ to 1 cm in diameter. Each branch carried three to five mature leaves. The fresh material was frozen in liquid nitrogen and broken into fine fragments while in the frozen state. Care was taken that the material remained frozen until it had been thoroughly dried in a laboratory vacuum lyophilizer. After lyophilization, the material was placed in sealed containers and stored in a deep freeze at −20°C.

The lyophilized material was extracted with absolute methanol under a partial vacuum sufficient to maintain the temperature of boiling methanol between −5° and −10°C. A modified Soxhlet apparatus was used with a condenser containing a dry ice-acetone slurry as a coolant. The methanol solvent was removed from the extract by evaporation at room temperature or below in a Rinco apparatus attached to a water aspirator system. The product was a dark green, tarry residue which was mixed to homogeneity with anhydrous (USP) lanolin.

Two grams of the residue mixed with 17 g of lanolin was applied to the underside of the leaf surface of ten test plants. Ten control plants were treated in like fashion with 17 g of pure lanolin. During the 14 days subsequent to the application of the lanolin preparations, all test plants and control plants were maintained on a precise 8-hour dark and 16-hour (500 ft-ca intensity) light regime. On the 14th day after application, the terminal bud of each plant was dissected to ascertain the flowering response. The flowering stages were numerically evaluated (*6*), and the results are presented in Table 1. Three earlier experiments had yielded essentially similar results.

It is our belief that these results constitute the first reproducible demonstrations of floral initiation in a short-day plant as the direct result of an extract prepared from the tissues of flowering plants. It is suggested that this is a crude extract of the flowering stimulus. Currently, efforts are being directed toward further concentration and characterization of the active entity in this extract (*7*).

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6. Results were evaluated in accordance with a numerical scale based on the diameter and morphological stage of development of the terminal staminate inflorescence. Vegetative plants were rated as zero on the scale. The first morphological change in the stem apex that could be clearly recognized as flowering was assigned a value of 1.0. A flowering apex measuring 0.25 mm in diameter was evaluated as 2.0. An additional increment of 1.0 was allowed for each 0.25 mm increase in the diameter of the developing inflorescence.
7. This investigation was supported by National Science Foundation grants (Nos. 7252 and 11482). We gratefully acknowledge the technical aid and assistance of Marda L. West and Donald Siems.

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Group and Phase Velocities for Rayleigh Waves of Period Greater than 380 Seconds

Abstract. Recent theoretical and experimental investigations of the periods of free spheroidal oscillation of the earth have shown good agreement. These data are used to determine group and phase velocities for Rayleigh waves of period greater than 380 seconds. The velocities so obtained are compared with those determined from analysis of progressive waves. It is concluded that group and phase velocities determined by either of the two methods are in agreement.

Phase velocity, C, and group velocity, U, may be determined from periods of free oscillation according to Jeans' formula (*1*):

$$C = 2\pi a/(n + \tfrac{1}{2})T$$

where a is the radius of the earth, n is the mode number of the oscillation, and T is the period. Periods of free oscillation of the earth have been determined by spectral analysis of the following records: Los Angeles gravimeter records (*2*), the Isabella (*3*) and Ogdensburg (*4*) strain seismograms, and the Chester (*5*), Palisades (*4*), and Pasadena (*3*) long-period pendulum seismograms. The theoretical periods for spheroidal (*6, 7*) and torsional (*8*) oscillations were used to identify peaks of the spectra. The different measurements show good agreement with each other and with theoretical calculations. Phase velocity points derived from the period of each free spheroidal mode from $n = 27$ to $n = 6$ are shown in Fig. 1 as a function of wavelength,

$$2\pi a/(n + \tfrac{1}{2})$$

It has been shown that this definition of wavelength is consistent with analysis of progressive wave trains provided a $\pi/2$ phase advance at each polar or antipodal crossing is taken into account (*9*). Two phase velocity points derived by such analysis (*9, 10*) are shown as open triangles in Fig. 1. The phase velocity curve reaches a maximum of 6.67 km/sec at a period of 700 seconds and then decreases because of the effect of the earth's fluid core.

The phase velocity curve shown in Fig. 1 was differentiated to give group velocity, U. This group velocity curve rises steadily from 4.0 km/sec at a wavelength of 2000 km ($T = 354$ seconds to 7.83 km/sec at a wavelength of 6200 km ($T = 967$ seconds), giving velocities much higher than those previously reported (*11, 12*), and denying a flattening of the Rayleigh wave group velocity curve beyond a period of 380 seconds. The previously reported flattening of the group velocity curve resulted from attributing certain observed long-period Rayleigh waves to the wrong wave trains. Thus a wave of period 480 seconds was previously identified as part of the wave train R10, giving a group velocity of 4.12 km/sec (*11*), but it gives a velocity of 4.97 km/sec when assigned to the train R12 (Fig. 1). Similarly, a velocity of 4.13 km/sec for a wave of period 630 seconds (*12*) identified with R18 increases to 6.02 km/sec when the wave is assigned to R26. A wave of period 530 seconds on the Chester, New Jersey, record of the Chilean earthquake gives a velocity of 5.24 km/sec when assigned to the train R18 (*13*).

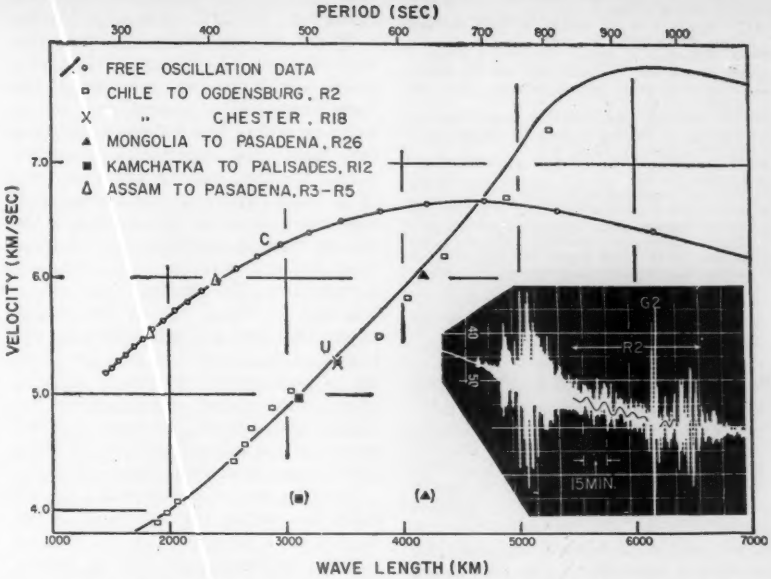

Fig. 1. Phase and group velocity data for mantle Rayleigh waves plotted as a function of wavelength with variable period scale at the top. Previous results, in error because of wrong identification of wave train, are shown in parentheses. The insert shows the Ogdensburg strain seismogram of the Chilean earthquake. The arrival time of the first clear event on the record, the S wave from a foreshock, is 19 18 00, 22 May, GMT.

Static load (*14*) and particle motion (*7*) calculations indicate that the supposed flattening could not be due to the earth's fluid core as had been suggested (*11, 12*). Jeffreys (*15*) also maintained that the core could not cause the flattening.

The Ogdensburg strain seismogram of the Chilean earthquake (Fig. 1, inset) recorded waves of periods near 700 seconds, preceding the G wave. These waves are interpreted as part of the train R2 with an epicentral distance of 31,280 km and give group velocities ranging up to 7.30 km/sec; these group velocities are in good agreement with those determined from periods of the free oscillations. Because of the high group velocities, this train had been tentatively identified as a higher torsional or spheroidal mode (overtone) of oscillation (*4*), but spectrum analysis of the strain seismogram for several days after the Chilean earthquake showed that the fundamental spheroidal type of motion with periods from 400 to 800 seconds was present with high amplitude on this record and therefore the long period waves shown in Fig. 1 are almost certainly due to this type of motion.

Thus analysis of seismograms in terms of standing waves or progressive waves gives comparable results. Progressive waves yield information which may be more readily associated with a given part of the earth than that from standing waves (*16*).

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9 February 1961

Pressure Sensitivity of an Amphipod

Abstract. The responses of an intertidal amphipod indicate an ability to perceive rapid pressure changes of less than 0.01 atm. The interaction of rate of change of pressure and total difference in pressure implies a rapid accommodation as well as a threshold. Evidence suggests that the sensory mechanism may not involve compressibility of a gas.

It has long been recognized that changes in hydrostatic pressure, in the range of hundreds of atmospheres, can have marked, and often lethal, effects on organisms (*1*). Recently, Hardy and Bainbridge (*2*) described controlled experiments in which decapod larvae slowly swam upward after much smaller increases of hydrostatic pressure: 500 to 2000 mb (*3*). The response continued for more than an hour. Two other recent papers have claimed that planktonic invertebrates, including representatives of several phyla, may be sensitive to changes in hydrostatic pressure as small as 10 mb. Knight-Jones and Qasim (*4*) described a "negative geotaxis" associated with pressure increase, and a general decrease in activity associated with pressure decrease. Baylor and Smith (*5*) stated that the organisms they examined swam upward the appropriate distance to compensate for pressure increase, and downward to compensate for pressure decrease. Neither of these reports described the experimental methods used to determine the presumed thresholds.

This preliminary report (*6*) deals with the brief but sharp increase in activity shown by a benthic intertidal amphipod, *Synchelidium* n. sp. (*7*), in response to small changes in hydrostatic pressure. The experimental equipment consisted of: a 250-cm³ observation flask, containing about 300 amphipods, attached to a 50-cm³ syringe by 6 m of 3-mm (inside diameter) plastic tubing, the entire system being filled with seawater, except for a 5-cm³ airspace in the flask; a transducer-type pressure gauge (Statham model PM 6 TC, ± 1 lb/in.² full range) connected directly to the flask by a glass Y-tube; a 30-cy/sec amplifying recorder (Gilson Medical Electronics, "Mini-polygraph") with one pen connected to the transducer and a second pen to an event-recording line leading from near the flask. The observation chamber, pressure gauge, and an observer were in a room where a high noise level was maintained; the recorder and the syringe through which pressure changes were applied were in a separate, adjacent room. The observer thus had no auditory or visual cue to the timing of pressure changes, other than the behavior of the animals. The large number of amphipods was used in order to allow more precise differentiation between the normal, average activity pattern of the animals and the increased activity associated with pressure changes.

Pressure on the system was changed either by placing (or removing) small weights on a platform atop the syringe plunger, producing an almost instantaneous pressure change; or by mechanically raising the open syringe up an inclined ramp at a rate that could be adjusted as desired. The air space in the flask is not essential to the response of the amphipods; they reacted similarly when the system was completely filled with water. It did serve, however, to considerably cushion the pressure change, eliminating large overshoot and rebound with the first method, and producing a much smoother pressure record with the second method.

The observer was notified that a recording had been started; at sometime within the next 60 seconds, the timing being determined by a number selected from a table of random numbers, the operator changed the pressure on the system. When, and if, the observer noted a marked change in the behavior of the amphipods, his signal depressed the event-recording pen of the recorder.

Several considerations make it appear unlikely that the observed response was due to a pressure-correlated sound stimulus: (i) The behavior of the animals when subjected to a wide variety of shock and noise stimuli, in the absence of pressure changes, indicates that this species is relatively unresponsive to all sounds tested, including loud, strong blows on the syringe. (ii) The absence of the air space in the experimental system, which should have altered the sound transmissive characteristics, had no noticeable effect on the response. (iii) The same type of response was obtained when the open syringe was raised carefully by hand, either with or without the airspace in the flask, a procedure presumably avoiding any high-frequency mechanical shocks associated with other methods. (iv) There is no known mechanical peculiarity of the system which could account for a sound stimulus present during prolonged linear pressure increases and absent during briefer increases (Table 1).

Table 1. Responses to slow, linear pressure increases: av. rate, 2.8 mb/sec (2.6 to 3.1); percentage of time at atmospheric pressure, 61 and 77; three "spontaneous activity" records. For $\triangle P$, $\triangle P_T$, and T, see Fig. 1*B*.

$\triangle P$ (mb)		Responses		T (sec)	
Av.	Range	Positive	None	Av.	Range
Large increases (final $\triangle P > 35$ mb)					
41.8*	27–51*	19	1	14.4	9.4–17.7
Small increases (final $\triangle P < 25$ mb)					
18.7	18–21	0	19		

* $\triangle P_T$

The system has been carefully examined for the possibility of some pressure-correlated visual cue to the animals; none has been found.

The most evident response to pressure change is the sudden onset of a rapid scrambling and darting by the amphipods, which are otherwise far less active, predominantly resting on the bottom of the flask. The reaction occurs whether the pressure is increased or decreased, but is much stronger after pressure increase. With slow rates of pressure increase, the sudden burst of activity is preceded by a brief reduction of the slight normal movements of the animals. This decreased activity is less easily discerned, however, and the criterion for pressure response will here be restricted to the sudden pronounced increased activity. Rough estimates of the duration of increased activity associated with pressure change ranged from about 5 seconds for an increase of 10 mb, to about 15 seconds for an increase of 100 mb.

Small bursts of spontaneous activity occasionally occurred in the observation flask; signals denoting an apparent response to pressure at a time prior to

Table 2. Threshold for observable reaction to rapid pressure increases: t, 0.6–0.9 sec; percentage of time at atmospheric pressure, 86 and 92; one "spontaneous activity" record. For $\triangle P$, T, and t, see Fig. 1*A*.

$\triangle P$ (mb)	Responses		T (sec)	
	Positive	None	Av.	Range
Initial threshold				
6–8	6	4	1.7	1.0–2.3
8–15	5	1	1.3	1.0–1.7
15–20	4	0	1.1	1.1–1.2
Final threshold				
6–8	0	5		
8–10	1	4	2.0	
10–15	8	4	1.9	1.5–3.0
15–20	5	0	1.6	1.4–1.8

the actual pressure change were designated as "spontaneous activity" records. These errors were made only when dealing with the weak responses near threshold level. Rapid pressure changes of more than 30 mb are associated with such an emphatic response that it cannot be confused with spontaneous activity. The decision that there has been a "sudden increase in activity" is, of course, subjective; the precision in timing relative to the randomly timed pressure changes, the reproducibility of results, and the low frequency of "spontaneous activity" records suggest, however, that activity changes associated with pressure changes are great enough to provide an objective basis for study of the phenomenon.

Pressure increases during the "active" phase of the animal's endogenous tidal rhythm (*8*) result in some increase in the number of upward-swimming animals, after the rapid scrambling on the bottom of the flask, a result similar to the "negative geotaxis" referred to by Knight-Jones and Qasim (*4*). This feature is not conspicuous during the "inactive" phase of the rhythm, even with large pressure increases, and has not been observed after pressure decreases at any time. Those animals which swim upward rise a distance of only 2 to 5 cm, regardless of the magnitude of the pressure increase; these benthic amphipods do not appropriately "compensate" in the manner described by Baylor and Smith (*5*) for planktonic organisms.

Tracings from typical experimental records are shown in Fig. 1. More than 50 experiments have been conducted with rapid pressure changes of 25 to 50 mb (Fig. 1*A*) with 100 percent positive responses and no "spontaneous activity" records.

Tables 1 and 2 summarize a representative set of experiments with pressure increase performed on 13 September 1960 from 9 A.M. to 1 P.M. during the "inactive" phase of the amphipod's tidal rhythm. The same group of animals was used throughout, with a minimum of a 1-minute interval between trials. The initial threshold experiments of Table 2 preceded the experiments in Table 1; the final threshold experiments followed them. The initial pressure, in all cases, was atmospheric ± 10 mb. The percentage of recorded time at atmospheric pressure is an index of the time during which "spontaneous activity" records were possible. Any responses to stimuli not associated with pressure changes would be expected to be distributed more or less evenly over the entire period of observation.

Table 2 indicates that the initial threshold for observable reaction was at least as low as 7.0 mb. Similar experiments with other freshly collected animals have shown positive responses down to a pressure increase of 5.1 mb. As a determination based on an overt reaction, this value should be regarded as only an estimated upper limit of true threshold.

Table 2 also indicates that the threshold to rapid pressure changes had increased during the experiments, to a value greater than 8.0 mb, but less than 15.0 mb. The increase in apparent threshold may be related to sensory fatigue from the intervening series of larger pressure increases. Control experiments indicated that reduced oxygen tension and other factors associated only with the experimental confinement did not measurably increase threshold.

Table 1 indicates that the response of the amphipods is dependent on both the total change and the rate of change of pressure. The large and small pressure increases of this series were presented in a random sequence. At a rate of change of about 2.8 mb/sec, an accumulated pressure difference of 18 to 21 mb did not evoke the response ("small increases"), compared with positive responses to less than 10 mb given rapidly (Table 2). The same rate (2.8 mb/sec), continued for 10 seconds or more, reproducibly induced response ("large increases"). The nature of the interaction of rate of change of pressure with total change of pressure is similar, at least superficially, to the properties of other better-known sensory systems.

The time between the start of pressure increase and the observation of a response (T) has a far greater range in Table 1 than in Table 2. In part, this is due to the fact that there was a significant trend ($p < .001$) for the value of T to increase during the course of the experiments. The trend may be another aspect of the sensory fatigue suggested by a comparison of initial and final thresholds (Table 2). In addition to this trend, however, the timing of *sequential* positive responses was also less precise than with rapid pressure changes. The difference in the value of T for successive responses averages 2.4 seconds (ignoring sign), which is greater than the entire range in Table 2. With slow pressure changes, interindividual variability in both the threshold pressure difference and the accommodation rate could contribute to a less sharp and coordinated response in a population; these factors would not alter the timing of initial response to rapid pressure changes.

While a gas-containing organ seems the most likely basis for a pressure-responsive sensory mechanism, there is, so far, no evidence for the presence of such an organ: (i) Extensive micro-

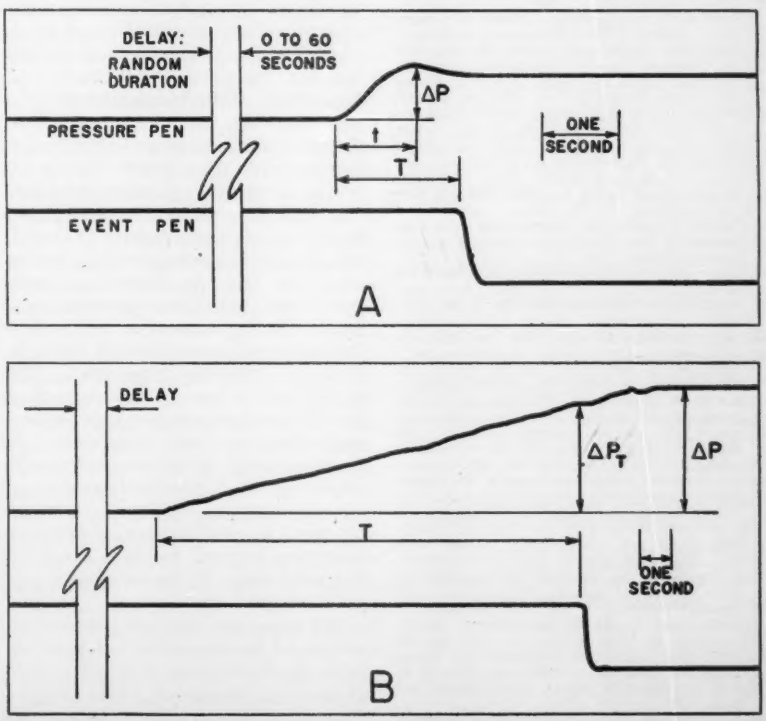

Fig. 1. Tracings of records from typical experiments. *A*, rapid pressure change; *B*, slower, linear rate of change. Symbols as used in Table 1.

scopic examination of the animals has not revealed any visible gas bubble. (ii) Upon evacuation of a flask of amphipods in sea water, any gas contained in a flexible organ might be expected to greatly expand and thus increase the buoyancy of the animals, but no change in buoyancy was observed on evacuation. (iii) After the animals were maintained at several atmospheres (about 4000 mb) for 10 minutes, they showed an activity response to pressure increases of as little as 30 mb above the 4000-mb level, even though one might expect a marked reduction in the size of a contained gas bubble, due to both compression and solution of the gas in the animal's tissue fluids, with probably an accompanying larger loss in sensitivity to pressure changes than that observed.

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6. This work was performed during the tenure of a National Science Foundation predoctoral fellowship. Experimental work was supported by National Science Foundation grant No. G-7141 to Dr. E. W. Fager. I wish to express my sincere thanks to my scientific adviser, Dr. E. W. Fager, for much helpful discussion and encouragement with the work. Thanks are also due to Richard Ford and Nancy Enright for their time as observers and operators in the experimental work, to Frank Snodgrass and James Snodgrass for help with instrumentation, to Dr. Robert Arthur for a helpful discussion of methods, and to a large number of other friends for valuable comments and discussions.
7. This small oedicerotid amphipod, which is abundant on the intertidal beach at Scripps Institution of Oceanography, La Jolla, California, was identified by Dr. Clarence Shoemaker (deceased) and will be formally described by Dr. Thomas E. Bowman in a forthcoming monograph on the genus. It differs from two other undescribed species of the genus found in the La Jolla area in the following conspicuous morphological characters: the first and second antennae of the mature male are roughly 80 and 95 percent, respectively, of the length of the cephalothorax; those of the female are roughly 45 percent each; comparable values for the other two species are about 50 and 220 percent for the male and 35 percent each for the female. The second and third uropods, in both sexes, are roughly 65 and 35 percent, respectively, of the length of the first uropod, compared with 80 and 65 percent for the other two species. The bulk of the adult population of this species is in the uprush zone of the beach at La Jolla; of the other two species, which are much less abundant, one ranges from low intertidal to depths of about 3 m, and the second is usually taken deeper than 5 m.
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12 October 1960

Primate Taxonomy and Oreopithecus

W. L. Straus, Jr., and M. A. Schön have reported their findings on the cranial capacity of *Oreopithecus bambolii* (*1*). The abstract from this report reads: "From a plaster reconstruction of the skull of the August 1958 skeleton, the cranial capacity of *Oreopithecus bambolii* has been estimated as falling between 276 and 529 cubic centimeters, thus within the ranges of variation of both orangutan and chimpanzee. In cranial capacity, therefore, and probably in body-brain ratio as well, *Oreopithecus is a hominoid*" (italics mine).

The authors explain that after Hürzeler's recent revaluation it seems evident that *Oreopithecus* is a member of the superfamily Hominoidea, which comprises the families Pongidae (anthropoid apes) and Hominidae (man and his immediate forerunners).

Three letters appeared last year in *Man* (*2*, *3*). Their titles clearly indicate the worry of anthropologists about the thorny problem of the taxonomy of our probable or possible ancestors.

Before *Oreopithecus* "revalued"—as Huxley would most likely say—came into the picture, Simpson's (*4*) creation of the superfamily Hominoidea appeared convenient, and thus the term has been widely used. We had "hominoid," "hominid," and "pongid" equated, as the adjectival forms of the zoological groups Hominoidea, Hominidae, and Pongidae. As Wells (*2*) has commented, "This nomenclature commits us to the view that man is more directly linked in origin with the anthropoid apes than with the other Old World primates (Cercopithecoidea). Although this conception is both plausible and widely held, Straus (1949) (*5*) has argued persuasively for the alternative view that the Hominidae arose from stem Catarrhines too primitive to be classified as anthropoid apes even in the broadest sense. On this view, the Catarrhina (*sensu* Hemprich, 1820) would form a natural unit with three co-ordinate subdivisions: Cercopithecidae, Pongidae and Hominidae; *the Hominoidea of Simpson would then be an arbitrary grouping. Nevertheless, at this stage anyone who proposes to use the term 'hominoid' in any other sense than that adopted by Le Gros Clark* (*6*) *must define its meaning very carefully*" (italics mine).

But thus far, although several ancestors of the Pongidae had been discovered corresponding to pre-Pleistocene chronology, hominids going beyond Pleistocene dating were unknown. Le Gros Clark could say in 1955 (*7*), "Similarly, the fossil Hominoidea of Miocene age may appropriately be called 'primitive anthropoid apes,' even though they had not acquired all the specialized features which are accepted as characteristics of the anthropoid apes of today."

That is, the use of the term hominoid "in any other sense than that adopted by Le Gros Clark" (*2*) seems to me, in the light of the recent *Oreopithecus* discoveries, as somehow unfitting. One may ask, Is this Upper Miocene primate a "hominoid" or a "primitive anthropoid ape," considering that he is classed under the superfamily Hominoidea? Then would *Proconsul* also be a "hominoid," and *Limnopithecus* another?

I believe that, as has been stated by several authors, while it was appropriate—in spite of Straus (*5*)—to create a superfamily which would encompass the two families Pongidae and Hominidae, it was unwise to give it a name which refers exclusively to one of them, and to that carrying a deeper emotional charge.

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17 October 1960

Dr. Santiago Genovés objects to the use, by myself and M. A. Schön (*1*), of G. G. Simpson's term, Hominoidea (and its adjectival and substantival derivative, "hominoid") to denote a superfamily comprising the families Pongidae (anthropoid apes) and Hominidae (man and his immediate forerunners). Consequently, he disapproves of our classification of *Oreopithecus* as a hominoid. In this, he is tilting with a windmill.

My paper of 1949 (*2*) has been cited by Genovés in his argument against use of the terms Hominoidea and hominoid. The implication is that my present acceptance of Simpson's definition of a superfamily Hominoidea contravenes my earlier views respecting man's ancestry, for this superfamily associates the anthropoid apes and man, with the Old World monkeys excluded as a separate catarrhine superfamily,

the Cercopithecoidea. It is true that I was not entirely happy about Simpson's classification of the catarrhine Primates when it appeared in 1945 (*3*). Simpson believed that "the usual diagnostic characters" justified the union of man and anthropoid apes in a single family; although in actual practice he placed them in separate families, Hominidae and Pongidae. Hence his superfamily Hominoidea seemed to denote an unreasonably late time of divergence of the hominid and pongid evolutionary lines. Since this feeling still persisted when I wrote my 1949 paper, I did not follow Simpson's taxonomy. Subsequently, however, this objection has come to appear unimportant, although I still believe in a relatively early separation of the hominid and pongid branches. Thus I now can see no good reason not to follow Simpson's classification. On the whole, its excellence cannot be denied. Moreover, since it has gained general acceptance, to use it is to avoid possible misunderstanding. I differ from it in one detail respecting the Hominoidea, however, in that I believe it is logical to recognize two families of anthropoid apes, Pongidae and Hylobatidae, rather than include all apes in a single family, the Pongidae. Yet this is perhaps largely a matter of personal taste. It does not alter the general validity of Simpson's taxonomy.

In any event, I still adhere to my views expressed in 1949, that the hominid and anthropoid-ape evolutionary lines separated, at a relatively early date, from a *"common ancestral stock"* (italics mine) which was "far more monkey-like than anthropoid-like." This means, moreover (and this perhaps is the major issue), that the Hominidae did not arise from animals which had undergone the specializations accompanying adaptation to brachiation but, rather, from animals which were "essentially unspecialized, monkey-like quadrupeds" and hence not "actual" anthropoid apes. But I did not then, any more than now, deny a common origin to the Hominidae and the anthropoid apes exclusive of the cercopithecoid monkeys. Indeed, I explained many of the resemblances between man and the great apes as the results of parallel evolution. This implies, as Genovés should know, inheritance of common genetic potentialities from common ancestors. A common ancestry for hominids and pongids (to the exclusion of the cercopithecoids) also is clearly expressed in the "family tree" which I then proposed (Fig. 8B) and to which I still adhere (except that I am less inclined than I was 11 years ago to branch off the hominid line before that of the gibbons). Thus, *pace* Wells and Genovés, there really is nothing in my views of 1949 against combination of the families Hominidae and Pongidae, together with their immediate common ancestors, in a superfamily, the Hominoidea. The sole possibility of my disagreeing with other workers, beyond the brachiation question, relates to the point of divergence of the Hominidae from the ancestral hominoid stock. This, however, necessarily remains a matter of opinion because the requisite fossil evidence is lacking.

Of the three letters in *Man* to which Genovés refers, only that of Wells (1959) is truly pertinent to the question which he raises. Moreover, by omitting the opening sentence of the paragraph cited from Wells' paper, he has failed to make it clear that Le Gros Clark's use of the term "hominoid" derives from Simpson's Hominoidea. Even if one accepts Clark's 1955 diagnosis of all the then known Miocene Hominoidea as "primitive anthropoid apes," it does not necessarily follow—as Genovés naively assumes—that *Oreopithecus* (which was but poorly known at that time) automatically falls into that category. To label *Oreopithecus* (which, incidentally, is generally regarded as Lower Pliocene in age; not Upper Miocene, as Genovés states) a "hominoid" is merely to state superfamily assignment. It does not involve allocation to any particular family of the Hominoidea. One does not imply that the skunk, *Mephitis*, is a dog by including it within the superfamily Canoidea. The same sort of thing applies when "hominoid" is attached to both *Proconsul* and *Limnopithecus*.

Although Genovés apparently is resigned to recognition of "a superfamily which would encompass the two families Pongidae and Hominidae," he protests against giving it *"a name which refers exclusively to one of them"* (italics mine). In this he exhibits innocence of established zoological taxonomic procedure. A noteworthy example of similar taxonomic artlessness has been cited by Simpson (*4*), who wrote: "Dart's placing of † *Australopithecus* in a family 'Homo-simiadae' (1925) only served to exemplify the total ignorance of zoology so common among the special students of these higher primates (although, of course, Dart's work is excellent in his own field)." The name of a higher category, whether it be that of a superfamily, family, or subfamily, is derived from that of *one* valid genus. In consequence, it must be "exclusive," since it cannot be *compounded* from the names of lower categories. Although a superfamily name is derived from that of a genus, not from that of a family, in common use its root is the same as that of *one* of its valid families. Consequently, one may ask, would Pongoidea (or Hylobatoidea) carry a less profound "emotional charge" than Hominoidea? Or, if one could employ the name of any available valid genus, would Gorilloidea, Panoidea, Symphalangoidea, Dryopithecoidea, or the like, be any more dispassionate?

I can think of only one possible solution which might satisfy those whose emotions are unduly aroused by the term Hominoidea. Since it now appears that the dentition of the Oligocene catarrhine primate, *Propliopithecus*, is more generalized than was originally thought, so that its classification as an actual pongid (or hylobatid) may well be questioned, this genus could be regarded as the type of the family, Propliopithecidae, which gave rise to the Pongidae (and Hylobatidae) and Hominidae. Accordingly, Propliopithecoidea could supplant Hominoidea as the superfamily name embracing all of these families. I seriously doubt, however, that this suggestion is likely to gain anything like general acceptance.

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27 December 1960

Facultative Heterotrophy in Some Chlorococcacean Algae

Abstract. All known species of the genera *Bracteacoccus, Spongiochloris,* and *Dictyochloris,* and some of the species of *Neochloris* and *Spongiococcum* are capable of growing heterotrophically in darkness in a glucose-salts medium. In contrast, all known species of *Chlorococcum* are obligate photoautotrophs. Possible relationships between these results and certain morphological characteristics are discussed.

Since the studies of Bristol-Roach (*1*), numerous investigations have dealt with facultative heterotrophy (that is, organotrophy) in algae. Especially significant in this area are the works of Petersen (*2*), Saunders (*3*), von Ernst and O. Pringsheim (*4*), Lewin and Lewin (*5*), and Belcher and Miller (*6*). Recently the family Chlo-

rococcaceae, which includes the green, unicellular, nonmotile, spherical zoospore-producing algae, has been reexamined, revised, and augmented with new species and genera (*7–10*). It now contains 11 or more genera and over 50 species, most of which are available in bacteria-free culture. This report concerns the results of studies of the distribution of facultative heterotrophy and presumptive obligate photoautotrophy within eight of these genera.

A loopful of inoculum, taken from an actively growing culture on a 1.5 percent agar slant containing complete inorganic salts medium B-1 (*11*), was transferred into liquid B-1 medium enriched with either glucose (0.20 or 0.50 percent) or a substituted carbon source. Other carbon sources used were sodium acetate, galactose, arabinose, glycerol, and glucose-1-phosphate, all except the acetate being added aseptically to the medium after autoclave-sterilization. Six flasks, each containing 50 ml of sterile medium, were inoculated with each alga. Three of these were illuminated under routine culture conditions (23°C, 300 ft-ca, 12-hour diurnal cycle of illumination); the remaining three were stored in darkness. After 2 or 3 weeks, flasks were examined immediately for visible growth.

Fourty-four species of algae were investigated. These are listed in Table 1, together with the results of the heterotrophic growth studies. It is noteworthy that all species of *Chlorococcum* examined were obligately photoautotrophic while all species of *Bracteacoccus*, *Spongiochloris*, and *Dictychloris* were facultatively heterotrophic at least with respect to glucose. Differences in the responses of separate species occurred in the genera *Neochloris* and *Spongiococcum* and were consistent. Species of *Radiosphaera* and *Nautococcus* were, like *Chlorococcum*, incapable of visible growth in glucose-salts medium, but they grew poorly in the illuminated cultures as well. Therefore, of the 44 cultures tested, 24 were capable of growth and 20 were apparently not. The actual extent of growth and pigmentation varied from one species to the next. Glucose was clearly the best substrate of those tested, and in many cases the presence of this particular carbon source was a *sine qua non* for obtaining significant heterotrophic growth.

Table 1. Chlorococcacean algae and their abilities to utilize glucose and acetate as separate carbon sources for growth in darkness; + indicates significant visible growth, − indicates none. The numbered isolates are available through R. C. Starr, Indiana Culture Collection, University of Indiana, Bloomington. The isolates marked with an asterisk (*) are stored temporarily at the Department of Botany, University of Texas, Austin, care of H. C. Bold.

No.	Species	Isolator	Glucose	Acetate
773	*Chlorococcum aplanosporum* Arce et Bold	Arce	−	−
950	*C. diplobionticum* Herndon	Herndon	−	−
118	*C. echinozygotum* Starr	Starr	−	−
972	*C. ellipsoideum* Deason et Bold	Bold	−	−
119	*C. hypnosporum* Starr	Starr	−	−
109	*C. macrostigmatum* Starr	Starr	−	−
117	*C. minutum* Starr	Starr	−	−
116	*C. multinucleatum* Starr	Starr	−	−
105	*C. oleofaciens* Trainor et Bold	T & B	−	−
775	*C. perforatum* Arce et Bold	Arce	−	−
774	*C. pinguideum* Arce et Bold	Arce	−	−
786	*C. punctatum* Arce et Bold	Arce	−	−
*	*C. scabellum* Deason et Bold	Bold	−	−
780	*C. tetrasporum* Arce et Bold	Arce	−	−
110	*C. vacuolatum* Starr	Starr	−	−
113	*C. wimmeri* Rabenhorst	Mainx	−	−
836	*Neochloris alveolaris* Bold	Bold	+	+
138	*N. aquatica* Starr	B. Norby	+	
754	*N. gelatinosa* Herndon	Herndon	+	
776	*N. minuta* Arce et Bold	Arce	−	
975	*N. pseudoalveolaris* Deason et Bold	Deason	+	
*	N. sp. (No. 4)	Parker	+	
*	N. sp. (No. 12)	Parker	+	
56	*Bracteacoccus cinnabarinus* (Kol et F. Chod.) Starr	Vischer	+	+
57	*B. engadiensis* (Kol et F. Chod.) Starr	Vischer	+	−
66	*B. minor* (Chodat) Petrova	Chodat	+	−
58	*B. terrestris* (Kol et F. Chod.) Starr	Vischer	+	−
*	B. sp. (No. A-20)	Parker	+	+
*	B. sp. (No. 9)	Parker	+	−
*	B. sp. (No. 1b)	Parker	+	−
*	B. sp. (No. 8b)	Parker	+	+
*	B. sp. (No. 3)	Parker	+	−
*	B. sp. (No. A-1)	Parker	+	−
*	B. sp. (No. T-1-2-6)	Bold	+	−
108	*Spongiochloris excentrica* Starr	Bold	+	
1	*S. spongiosus* Starr	Vischer	+	
977	*S. lamellata* Deason et Bold	Deason	+	
960	*Spongiococcum alabamense* Deason	Deason	+	
*	*S. excentricum* Deason et Bold	Kuehn	+	
976	*S. multinucleatum* Deason et Bold	Deason	+	
961	*S. tetrasporum* Deason	Deason	−	
33	*Dictyochloris fragrans* Vischer	Vischer	+	
460	*Radiosphaera dissecta* (Korsch.) Starr	Vischer	−	−
125	*Nautococcus pyriformis* Korschikoff	Starr	−	

Of the 20 presumptive, obligate, photoautotrophic species, 17 possess hollow, cup-shaped chloroplasts, 19 have walled zoospores, and all 20 have pyrenoids; of the 24 facultative, heterotrophic algae, 18 lack hollow, cup-shaped chloroplasts (that is, instead possess discoid, net-like, sponge-like, and so forth, ones), 21 have naked zoospores (that is, lack rigid cell walls), and 12 have pyrenoids (*8*). While there is clearly no perfect correlation between facultative heterotrophy and the separate generic (and specific) morphological criteria in this algal family, the possession of both a cup-shaped chloroplast and a walled zoospore, charateristics of the genus *Chlorococcum*, is unequivocally associated with species which appear to be obligate photoautotrophs. Similarly the possession of discoid, net-like, or sponge-like chloroplasts, in association with naked zoospores in the life cycle, appears to be linked with facultative heterotrophy. Such correlations, though perhaps fortuitous, may also occur in the Xanthophyceae, judging from the results of Belcher and Miller (*6*).

These data demonstrate that it is possible currently to distinguish all species of the genus *Chlorococcum* from those of *Bracteacoccus*, *Spongiochloris*, and *Dictyochloris* exclusively by their abilities to grow in darkness in liquid glucose-salts media; the ability to grow similarly on other carbon sources may be subservient for differentiation of species within the latter three genera. However, until the foibles of these correlations have been sought and tested more thoroughly, it is recommended that their application to the current system of classification be considered with reservation (*12*).

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11. The composition of B-1 medium is as follows: The concentration of macronutrients is identical to that described by H. C. Bold [*Bull. Torrey Botan. Club* **76**, 101 (1949)]; to each liter of this solution was added 1.0 ml of each of the following micronutrient stock solutions: EDTA stock: 50 g EDTA plus 31 g KOH/lit. H-Boron stock: 11.42 g H_3BO_3/lit. H-Fe stock: 4.98 g $FeSO_4 \cdot 7H_2O$/lit. acidified water (acidified water = 999 ml distilled water plus 1.0 ml concentrated sulfuric acid). H-H_5 stock (per liter of acidified water): $ZnSO_4 \cdot 7H_2O$, 8.82 g; $MnCl_2 \cdot 4H_2O$, 1.44 g; MoO_3, 0.71 g; $CuSO_4 \cdot 5H_2O$, 1.57 g; $Co(NO_3)_2 \cdot 6H_2O$, 0.49 g.

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Shear Rate Dependence of the Viscosity of Whole Blood and Plasma

Abstract. The analysis of the shear stress/shear rate relationship, and thus the viscosity/shear rate relation, of blood and plasma shows that (i) freshly drawn whole blood has a large shear rate dependence on viscosity (viscosity falls as shear rate increases), and (ii) the shear rate dependence of viscosity of whole blood, or plasma, that has not been treated to prevent clotting is substantially greater than that of whole blood or plasma treated with anticoagulants. The influence of this phenomenon upon the fluid mechanics of the microcirculation is commented upon.

In most of the studies (*1*) reported in the literature on whole blood or on blood plasma, the viscosity has been measured by a capillary viscometer. Among these studies are a number (*2*) in which the non-Newtonian or anomalous nature of the viscosity of whole blood was recognized, since the viscosity was observed to vary with the diameter of the capillary, the pressure gradient, the flow rate, and so on. On the other hand, possibly on account of the formidable difficulty in the interpretation of capillary data for non-Newtonian fluids, we have not as yet found an instance in which the apparent viscosity (defined as shear stress/shear rate) has been analyzed as an explicit function of shear rate.

In what appears to be a unique example of viscometry by means of a coaxial cylinder viscometer, Brundage (*3*) tested both whole blood and blood plasma and found the viscosity of blood to be mildly shear dependent (decreasing as shear rate increases), whereas the viscosity of plasma was found to be Newtonian (not dependent on shear rate). As the coaxial cylinder viscometer permits unequivocal determinations of shear rate as well as shear stress, it is possible to recalculate Brundage's data originally given as a function of rotational speed, in terms of absolute shear rate. Describing viscometric data in terms of shear stress versus shear rate, or apparent viscosity versus shear rate, seems highly desirable because it permits intercomparison of data from different instruments and by different workers, and it offers an absolute basis for the analysis of the circulation by fluid mechanics.

In most previous studies, the blood or blood plasma was rendered nonclotting prior to viscometry by defibrination, addition of heparin, addition of oxalate, and so forth. Brundage reports that his attempts to test whole blood were unsuccessful because clotting commenced during the viscometry; consequently he added oxalate. Pirofsky (*4*) obviated the use of anticoagulants by the use of an intravenous needle functioning as a capillary, but he was limited by the nature of this experiment to a one-point determination on the shear rate viscosity curve. The studies reported here resulted from a twofold objective: (i) to determine the rheology of human blood and blood plasma directly in absolute values of shear stress and shear rate; and (ii) to determine the rheology in so short a time after withdrawal from the subject as to be able to work directly with unmodified, untreated whole blood and plasma. As is shown below there appear to be substantial differences, respectively, between freshly analyzed blood or plasma and the same modified by anticoagulants.

The samples of blood obtained in this study were collected by venepuncture through a No. 18 needle (1-mm inside diameter) gently aspirated by a silicone-coated syringe. Anticoagulants when used consisted of 200 units of heparin of 20 mg of potassium oxalate per 10-cm³ volume of whole blood. The sample was placed in a plastic cup and analyzed at once by means of a cone and plate viscometer (*5*, *6*). In the experiments with plasma, the blood sample was first centrifuged in a plastic tube (3 minutes at 3400 rev/min), and the supernatant plasma was tranferred to the plastic cup and tested at once. The viscometer (*6*) used in this study was adapted from an industrial viscometer in which a cone was attached to a spindle projecting from the drive mechanism. The spindle was linked to the drive shaft by a calibrated beryllium copper spring to which a pointer was attached and was operable at four different speeds. The degree of deflection caused by the cone rotating in the fluid was read by the position of the pointer over a dial rotating in phase with zero position of the spring. All observations were carried out in a constant-temperature water bath at 37°C. A 3-ml sample of blood without anticoagulants could be fully analyzed within 3 minutes, and plasma within 6 minutes, from the time of collection. Clotting could easily be determined by sudden erratic movements of the pointer. Reproducible values could be obtained up to the instant of clot development, which appeared therefore to be a sudden rather than a gradual phenomenon, as determined by this technique. The relation of shear stress to shear rate of a National Bureau of Standards viscometer calibration oil (H-oil), with a viscosity of 4.635 centipoise at 37.0°C, is plotted in Fig. 1 to demonstrate that for a Newtonian fluid the viscometer correctly gives a linear relation between shear stress and shear

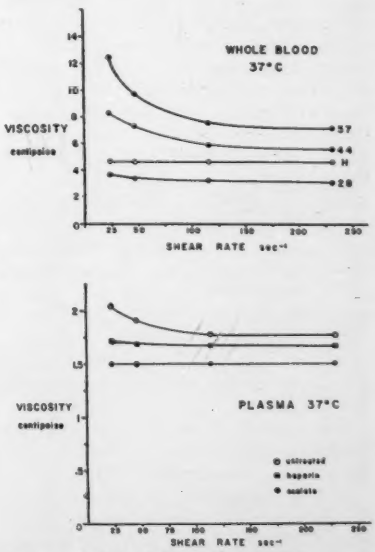

Fig. 1. (Top) Rheological diagram of whole blood with three different hematocrits; *H*, Bureau of Standards calibration oil. Fig. 2. (Bottom) Rheological diagram of three samples of plasma derived as noted. Serum protein, 6.9 g/100 g.

Table 1. A comparison of the coaxial cylinder data of Brundage on oxalate-stabilized whole blood (*3*) with our data for fresh, untreated whole blood, at equivalent hematocrits; cp, centipoise.

Hematocrit	Shear rate (sec^{-1})	Viscosity			
		Brundage		Present work	
		cp	A/B	cp	A/B
28	23 (A)	2.8	1.00	3.7	1.12
28	46 (B)	2.8		3.4	
44	23 (A)	6.0	1.00	8.4	1.15
44	46 (B)	6.0		7.3	
56	23 (A)	16.6	1.12	12.6	1.38
56	46 (B)	14.8		9.1	

rate, and that the instrument value agrees precisely with the stated viscosity.

A cone and plate viscometer is particularly appropriate for the study of blood rheology, because a small sample can be rapidly analyzed to obtain unequivocal and absolute values for shear stress and therefore viscosity for every value of shear rate imposed on the sample. The validity and derivation of the shear stress and shear rate with this type of instrument have been given elsewhere (*6*). In the present studies the full-scale deflection of the viscometer spring equaled 673.7 dy-cm, while the cone had an angle of 1°33′ and a radius of 2.4 cm. The four shear rates recorded, therefore, were 23.18, 46.36, 115.92, and 231.84 sec^{-1}.

In Fig. 1 (top) the curves of apparent viscosity of whole blood versus shear rate are presented for three hematocrit levels as indicated. The characteristic shear thinning type of curve for pseudoplastic fluids is apparent. The higher the hematocrit the more pronounced is the rise of apparent viscosity at low rates of shear. Values of serum protein and cholesterol were normal in all samples studied. To clarify the role of anticoagulants, the coaxial cylinder data of Brundage (*3*) on oxalate-stabilized whole blood are compared with our data (Table 1) for fresh, untreated whole blood, at equivalent hematocrits.

There appears to be a consistent trend in these data of substantially greater shear dependence of viscosity in fresh whole blood as compared to oxalate-treated whole blood, as indicated by the viscosity values at rates of 23 sec^{-1}, (A), and 46 sec^{-1}, (B).

In Fig. 1 (bottom) are shown curves of viscosity of plasma versus shear rate. The three curves represent values obtained with untreated plasma, and plasma obtained from anticoagulant-treated whole blood. All three were obtained from the same blood sample. It is evident that below approximately 50 sec^{-1} shear rate, the curvature (and thus the shear rate dependence of viscosity) of fresh plasma is greater than that of heparinized or oxalate-derived plasma.

In respect to plasma, Brundage (*3*) working on oxalate-treated samples found no difference in viscosity between a 2.2 sec^{-1} and a 5.14 sec^{-1} shear rate, which is in essential agreement with the findings reported here. Bingham (*7*) makes no mention of having observed non-Newtonian viscosity in plasma in which the content of oxalate-treated fibrinogen was varied. In the light of his pioneering work in non-Newtonian rheology, it is unlikely that non-Newtonian viscosity effects would have escaped his notice, had they existed.

The above data seem clearly to indicate that more attention should be focused on the viscosity of fresh blood and fresh plasma, even though it is experimentally difficult, in order to detect rheological characteristics at different rates of shear.

The consequences of shear rate dependence of viscosity on hemodynamics may prove to be profound. For example, in the microcirculation, the procession of erythrocytes in "single file" through capillaries (*8*) may be strongly influenced by the high shear rate dependence of plasma viscosity, in that next to the wall where the shear rate is highest, the plasma viscosity would be low, whereas between any two erythrocytes where the shear rate is very low, the viscosity would be high. Such a situation would tend to stabilize single-file procession and minimize tumbling or rotation of the erythrocytes. This shear rate dependence of blood and plasma viscosity does not necessarily support or contradict the concepts of "plasma skimming" or "axial streaming" (*9*) as applied to flow in the smaller vessels. Continuing research is aimed at elucidating the effects of the shear rate dependence of viscosity upon these and related phenomena.

Freshly drawn whole blood and fresh plasma separated from the red cells show a high degree of shear rate dependence of viscosity, whereas the addition of oxalate or heparin reduces the degree of shear rate dependence of viscosity in whole blood and causes it virtually to vanish in plasma. The shear rate dependence of viscosity in whole blood, especially in regard to movement of erythrocytes through arterioles, capillaries, and venules, would appear therefore to be a critical factor in the dynamics of blood flow of the microcirculation.

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23 November 1960

Faunal Remains on an Antarctic Ice Shelf

Abstract. Fishes and benthic invertebrates discovered frozen *in situ* and exposed at the ablation surface of the McMurdo ice shelf provide evidence both for the occurrence of a fauna, including large fishes, under the permanent ice shelf and for Debenham's hypothesis of the nourishment of an ice shelf by the freezing of sea water on its bottom.

On 8 November 1960 we found the remains of upwards of 50 partially decomposed fish and the remains of several kinds of benthic invertebrates on the surface of the floating ice shelf in McMurdo Sound, Antarctica (Fig. 1). The remains were scattered over a small area some 2 km from the ice front near the easternmost of the Dailey Island group (77° 52′ S, 165° 18′ E). The ice shelf is probably more than 30 m thick in this area, and the surface is at least 3 to 5 m above sea level.

The largest intact fish measured 142 cm over-all. Some detached heads appear to have come from still larger fish. Numerous pelecypods, gastropods, brachiopods, siliceous sponges, and anthozoan corals were also found on the ice surface. Even the fragile invertebrates were well preserved, and, in the case of some sponges and corals, they were still attached to rocks that presumably came with them from the sea floor.

The significance of the find is twofold: first, the fish are larger than any yet reported from the Ross Sea and McMurdo Sound; second, the fish with the benthic sessile invertebrates could scarcely have reached this place alive, the only possible access from the sea being the tidal cracks in the ice bordering the nearby island. Thus there is reason to believe that a well-developed fauna exists under the permanent ice shelf.

Published information on fishes from the Ross Sea and McMurdo Sound pertains to much smaller specimens, usually not longer than about 30 cm. Remains of somewhat larger fishes have been regularly observed after regurgitation by Weddell seals. A large headless fish was harpooned along with a seal in the sea during the National Antarctic Expedition (1901–04) which was quartered on nearby Ross Island. This specimen, which was 112 cm long without the head, was identified as a nototheniid, *Notothenia* sp. (*1*). The specimens observed by us on the ice shelf appear to be members of at least two genera of Nototheniidae, the most common family of antarctic fishes.

Debenham (*2*) reported finding the headless remains of a somewhat smaller fish on the ice shelf in the same area during the British (Terra Nova) Antarctic Expedition of 1910–13. He also reported finding perfectly preserved sponges and corals on the ice surface. He suggested that these might have been trapped in the ice by freezing when the bottom of the ice shelf touched the sea floor, and then were brought slowly to the top by the progressive melting of the upper surface while new ice formed at the bottom.

On the basis of evidence available to him, Debenham concluded that the ice shelf was nourished principally by the freezing of sea water on its bottom surface, and that the main body of the Ross Ice Shelf (5.18×10^5 km^2) might be nourished in a similar manner. Though it is now generally believed that the main body of the Ross Ice Shelf is nourished principally by the accumulation of snow on its upper surface, the finding of marine fauna apparently preserved for hundreds of years in the ice would lend support to Debenham's hypothesis, with respect to the McMurdo ice shelf where there is definite evidence of considerable surface melting. This method of nourishment of a permanent floating ice sheet of con-

Fig. 1. McMurdo ice shelf near one of the Dailey Islands showing ablation surface. Dark surface area is wind-blown volcanic debris. Fish are indicated by arrows. [National Science Foundation photograph by R. J. Litell]

siderable area and thickness would be unique, the only known parallel being the small ice shelf off the north coast of Ellesmere Island in the arctic. Accordingly, we have taken samples of vertebrae and flesh from the newly found fish for carbon-14 dating, together with several specimens for taxonomic identification. The tentative radiocarbon date appears to be up to 1100 years (*3*, *4*).

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2. F. Debenham, *Quart. J. Geol. Soc. London* **75**, 51 (1920).
3. Arrangements for radiocarbon dating were kindly made by F. A. McNeill, Scientific leader, New Zealand Antarctic Research Expedition. Dating was accomplished through the assistance of Dr. T. A. Rafter, director, Institute of Nuclear Sciences, Lower Hutt, New Zealand.
4. This study was supported by the National Science Foundation grants NSF-G12503 (University of Michigan) and NSF-G13209 (Stanford University) during 1960–61 operations of the U.S. Antarctic Research Program (Deep Freeze 61). The logistic aid by the U.S. Antartic Support Acitivities, Naval Air Facility, McMurdo Sound, Antarctica, and by Air Development Squadron Six (VX-6) is gratefully acknowledged.

28 November 1960

New Approach to Immunization against Schistosoma japonicum

Abstract. Previous inoculation of rhesus monkeys with cercariae of the nonhuman strain of *Schistosoma japonicum* proved to give a rather strong protection against a subsequent challenge with its human strain.

Investigators (*1*) have indicated that no definite resistance to second infection in schistosomiasis is present until the worms causing the primary infection have matured. It is probable, therefore, that the most important immunizing antigens are connected with schistosome eggs in the host. As the essential role in pathogenesis of schistosomiasis is ascribed to the parasite's eggs, it is regrettable to think that the immunity in schistosomiasis was produced by the reactions of an enormous number of detrimental eggs in the host's essential organs.

We have reported (*2*) that in *Schistosoma japonicum* there is a nonhuman strain which will naturally terminate its infection in the schistosomula stage in man, producing no important harmful effect during the course of infection. It will be of interest to investigate whether inoculations of the nonhuman strain of *S. japonicum* will produce immunity against the infection of its human strain. Also, it has been reported (*3*) that the rhesus monkey, *Macaca mulatta*, like man, is not susceptible to infection of the nonhuman strain of *Schistosoma japonicum*, although it is susceptible to the infection of the other human strains. Thus an immunization study was considered possible in rhesus monkeys without using human volunteers.

Table 1. Number of eggs of *Schistosoma japonicum* in the stools of immunized and nonimmunized monkeys.

Monkey No.	Inoculation: Immunizing No.	Inoculation: Immunizing Cercariae (No.)	Inoculation: Challenge Cercariae (No.)	Inoculation: Challenge Days after immunizing inoculation 1st	Inoculation: Challenge Days after immunizing inoculation Last	Eggs/gram per stool/day Maximum	Eggs/gram per stool/day Mean, first 30 days
			Immunized monkeys				
9	2	3,400	400	41	14	58	4
10	2	3,073	400	39	21	18	4
1	3	1,800	400	283	14	0	0
8	3	2,000	400	41	14	1,634	308
11	3	2,000	400	41	14	130	25
49	3	5,000	400	74	31	146	45
42	4	9,000	400	141	30	36	8
14	5	5,600	400	67	4	450	57
15	5	5,450	400	67	4	548	58
13	7	12,515	400	261	31	624	140
5	8	3,600	400	148	21	616	114
			Nonimmunized monkeys				
32			400			14,790	1,220
31			400			21,840	4,712
30			400			69,120	6,693
55			400			88,400	46,036

In our experiments, the Formosan strain of *S. japonicum* was used as the nonhuman strain and the Japanese strain as a human strain. Eleven monkeys were first inoculated by the cutaneous route with cercariae of the Formosan strain and then challenged with cercariae of the Japanese strain. Four monkeys were used as control animals which were inoculated only with the Japanese strain. While the number of cercariae of the Formosan strain given to each monkey varied, the number of the cercariae of the Japanese strain was always 200 males and 200 females for each monkey. Beginning from the 30th day of infection, the stools of each monkey were examined daily by the sedimentation method. After schistosome eggs were found in the stool, daily egg counts were followed for the entire patent period. As the maximum number of eggs per gram of stool per day during the patent period and the mean number of eggs of the first 30 days usually represent satisfactorily the intensity of infection of nonimmunized and immunized rhesus monkeys infected with *S. japonicum*, they were used as bases for the comparison in this paper (Table 1).

In the immunized monkeys, the maximum number of eggs per gram of stool per day in the patent period varied from 0 to 1634, while in the nonimmunized it varied from 14,790 to 88,400. The mean number of eggs of the first 30 days of the patent period in the immunized monkeys varied from 0 to 308, while in the nonimmunized monkeys the difference was from 1220 to 46,035. From these results, it can be concluded that previous inoculations of rhesus monkeys with the cercariae of the nonhuman strain of *S. japonicum* gave a rather strong protection against a subsequent challenge with its human strain. It is hoped that, by a proper adjustment of the timing, dosage, and number of inoculations of the immunizing cercariae, a state of absolute resistance to the challenge infection might be reached. The immunization procedure introduced here may hopefully lead to the development of an effective way to combat schistosomiasis japonica (*4*).

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2. H. F. Hsü and S. Y. Li Hsü, *Am. J. Trop. Med. Hyg.* **5**, 521 (1956).
3. ———, *J. Parasitol.* **46**, 228 (1960).
4. This investigation was supported by research grants from the National Institute of Allergy and Infectious Diseases, E-939 (C5), National Institutes of Health, U.S. Public Health Service, and from the Abbott Laboratories, North Chicago, Ill.

14 November 1960

Association Affairs

AAAS Membership

1) Changes during 1960		
New members elected		9,256
Losses		
Deaths	349	
Resignations	1,921	
Dropped for nonpayment of dues	4,181	
Total loss		6,451
Net increase during 1960		2,805
2) Totals as of 31 December 1960		
Annual members in good standing		56,283
Life and emeritus members		1,149
Total in good standing		57,432
In arrears		3,508
New for 1961		1,157
Total membership		62,097

Idaho Academy of Science

The Idaho Academy of Science, a new affiliate of the AAAS, was organized in 1958 to stimulate scientific education and research and the diffusion of scientific knowledge in the state of Idaho, to promote fraternal relationships among those engaged in scientific work, to assist in the development of the resources of the state, and to publish reports of scientific investigations pertaining to Idaho. Membership is open to all persons having an interest in science, and the membership roll currently exceeds 350. Among the various members are individuals from secondary as well as higher institutions of learning, government services, industry, agriculture, and the general citizenry.

Two publications, edited by Earl J. Larrison, Department of Biological Sciences, University of Idaho, are distributed. A newsletter, *The Retort*, appears four or five times a year and is devoted to academy news, forthcoming meetings, scientific activities in the state, and to items of general interest to members. The *Journal of the Idaho Academy of Science*, with two or three issues a year, contains papers reporting original research, as well as reference articles on the natural resources of the state. Some articles published to date have included such subjects as wildlife photography, plankton sampling techniques, rodents of Idaho, autotetraploids, paleogeology, skin diving in biological research, and machine retrieval of literature.

Aided by a grant from the National Science Foundation, the academy is presently conducting a series of visits by scientists to Idaho high schools for consultation and lecturing. This program is directed by Elmer K. Raunio, Department of Physical Sciences, University of Idaho.

Four subsidiary sections of the academy have been organized so far. These are the zoology section (Harry K. Fritchman, Boise Junior College, president), botany section (Lorentz Pearson, Ricks College, president), physical science section (Boyd Henry, College of Idaho, president), and science education section (Merlin Nelson, Coeur d'Alene High School, president). The latter section is currently preparing a report on a proposed junior academy of science to be affiliated with the senior academy.

Annual meetings include business, general address, and paper sessions. The election of officers and other matters relating to the general conduct of the academy are taken care of at the general business meeting. Much of the time spent at annual meetings is concerned with the delivery of research papers in the various sections. One feature of general interest is the series of "Idaho Academy of Science Symposia," one of which is given at each annual meeting by a panel of experts on some topic of state interest. Two such symposia have dealt with the Idaho phosphate industry and the Palouse region. The academy recently conducted a successful joint meeting with the Northwest Scientific Association at Moscow. The next annual meeting will be held 21 and 22 April 1961 at the College of Idaho, Caldwell. In addition to the paper sessions, major addresses will be given by Richard Doane, manager, Atomic Energy Division of Phillips Petroleum Co. (Idaho Falls); Miles Willard, research director, Rodgers Bros. Seed Co. (Idaho Falls); and Joseph Trainer, University of Oregon Medical School, Portland. The subject of the symposium will be "The Snake River."

Current officers are as follows: president, Albert E. Taylor, Idaho State College, Pocatello; vice-president, Donald J. Obee, Boise Junior College, Boise; acting secretary, Allen J. Hollenbeck, Boise High School, Boise; acting treasurer, William F. Zelezny, Phillips Petroleum Co., Idaho Falls. The executive committee consists of William H. Baker, University of Idaho, Moscow, past president; Lyle M. Stanford, College of Idaho, Caldwell, program chairman; J. E. O'Connell, National Science Foundation, Washington, D.C., academy conference representative; Earl J. Larrison, University of Idaho, editor; Gilbert Ford, Northwest Nazarene College, Nampa, membership chairman; and Gordon Dixon, Ricks College, Rexburg, and Malcolm M. Renfrew, University of Idaho, as appointed members.

J. E. O'Connell, assistant professor of botany, Department of Biological Sciences, University of Idaho, and currently on leave with the National Science Foundation, is the academy's representative on the AAAS Council and Academy Conference.

EARL J. LARRISON
University of Idaho, Moscow

Tool and Manufacturing Engineers

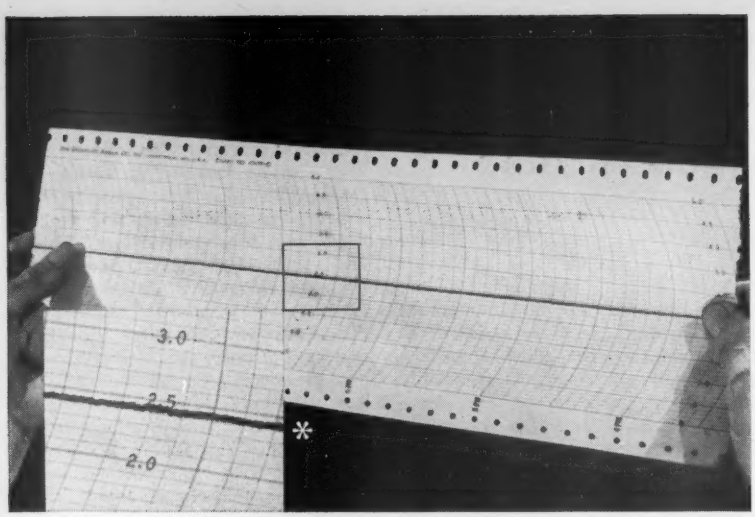
Affiliation of the 40,000-member American Society of Tool and Manufacturing Engineers (ASTME) with the AAAS has been unanimously approved by the Association's committee on affiliation, board of directors, and council.

The society, which was founded in 1932 in Detroit, currently has some 170 senior chapters—156 in the United States, nine in Canada, three in Australia, and single chapters in Mexico City and Manila. It also has 32 student chapters at various colleges and universities in this country.

Nearly all members are active in some field of manufacturing engineering. The specialties of individual members range from basic scientific research in machining, metal forming, and the like to practical application of that research in the design of a modern production line.

The society's primary objective is to encourage the development and adoption of more efficient manufacturing methods in industry. The scope of its current activities is illustrated by its programs for 1961.

The ASTME Research Fund, administered by a team of scientists and



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engineers who are in touch with industry problems, will make grants totaling many thousands of dollars to universities and research institutions for carrying out basic scientific research in fields that are related to manufacturing. Research problems covered are fundamental in nature, such as investigation of the basic nature of the metal-cutting process.

The results of the society's scientific research projects are published and made widely available to scientists and engineers. One such project that has attracted wide interest is the development and publication of a complete bibliography of metal cutting—a compilation of everything that is known about this subject. The bibliography is the result of more than four years of ASTME-sponsored research at the John Crerar Library, where experts reviewed and abstracted over 18,000 books, papers, and pamphlets on metal cutting.

The society also publishes scores of technical papers delivered at its meetings in the course of the year. These are believed to comprise the most complete selection of up-to-date information on the scientific-engineering aspects of manufacturing available anywhere.

The society's magazine, the *Tool and Manufacturing Engineer*, is distributed to all members. Its objective is to present scientific and technological developments of interest to its readers in readable and attractive form. During 1961, the 13 issues of the magazine will contain over 1200 pages of articles.

In the field of education, in addition to sponsoring student chapters at leading colleges and universities, the society continuously sponsors on-campus conferences, where engineers from industry keep in touch with research developments and educators refresh their knowledge of industry's problems and needs. There will be at least 30 of these conferences during 1961, with a total attendance of several thousands.

The society also sponsors numerous seminars where experts present the latest developments in specialized fields of manufacturing science to relatively small groups of scientists and engineers, who are able to pool their knowledge. It is expected that there will be about ten national seminars of this kind during 1961, with literally scores of local and regional seminars. All of the papers delivered at these seminars are published and widely distributed.

Each of the society chapters meets at least nine times a year. The meetings are primarily educational in nature. At a typical meeting, one or two speakers deliver papers on some specialized aspect of science or engineering. The members then participate in panel discussions and question-and-answer sessions. One of the real contributions of these meetings is the cross-fertilization

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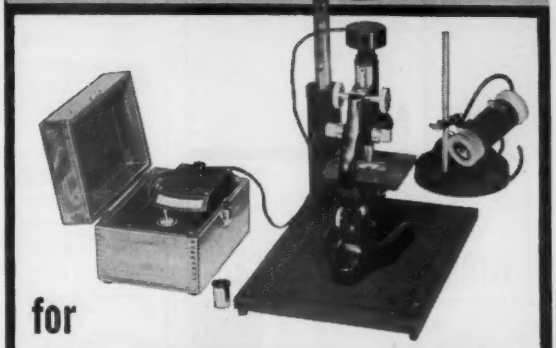

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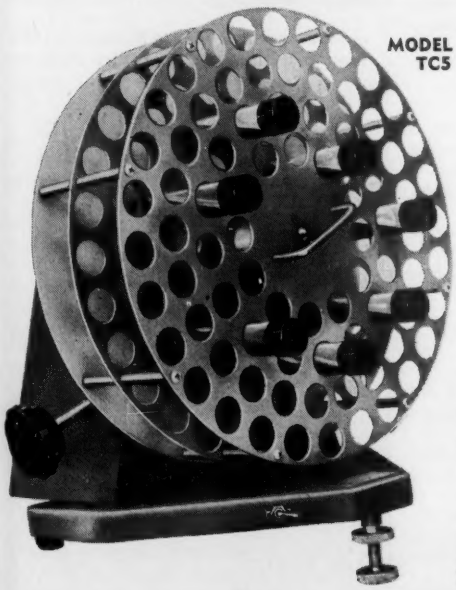

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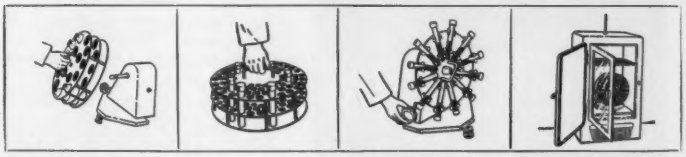

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of ideas that results when experts meet to discuss common problems. It is estimated that attendance at chapter meetings during 1961 will total over 200,000.

Each year the society holds an engineering conference and exhibit that attracts over 40,000 participants, who attend sessions on various aspects of science and engineering and visit some 6000 exhibits. The next such conference will be held in New York City, 22–26 May.

Harry Osborn, past president, will represent the society on the AAAS Council. He is research director of Tocco Division, Ohio Crankshaft Company, and is an internationally known authority on the heat treatment of metals.

HARRY E. CONRAD
American Society of Tool and Manufacturing Engineers, Detroit, Michigan

Forthcoming Events

April

9–13. American Assoc. of Cereal Chemists, annual, Dallas, Tex. (J. W. Pence, Western Utilization Research & Development Division, 800 Buchanan St., Albany 10, Calif.)

9–13. American Industrial Hygiene Assoc., Detroit, Mich. (W. S. Johnson, Bethlehem Steel Co., Bethlehem, Pa.)

9–15. American Institute of Nutrition, Atlantic City, N.J. (A. E. Schaefer, ICNND, Bldg. 16A, National Institutes of Health, Bethesda 14, Md.)

10–14. American Soc. of Civil Engineers, Phoenix, Ariz. (W. H. Wisely, 33 W. 39 St., New York 18)

10–14. Detection and Use of Tritium in the Physical and Biological Sciences, intern. symp., Vienna, Austria. (Office of Special Projects, U.S. Atomic Energy Commission, Washington 25, D.C.)

10–15. Federation of American Societies for Experimental Biology, 45th annual, Atlantic City, N.J. (M. O. Lee, 9650 Wisconsin Ave., Washington 14, D.C.)

10–15. Metallic Corrosion, 1st intern. cong., London, England. (Society of Chemical Industry, 14 Belgrave Sq., London, S.W.1)

11–13. Institute of Environmental Sciences, annual, Chicago, Ill. (H. Sanders, Box 191, Mt. Prospect, Ill.)

11–13. Ultrapurification of Semiconductor Materials, conf., A.F. Office of Scientific Research, Boston, Mass. (Miss H. Turin, Conf. Secretary, Electronics Research Directorate, Air Force Cambridge Research Lab., L. G. Hansom Field, Bedford, Mass.)

12–13. Information and Decision Processes, 3rd symp., Lafayette, Ind. (R. E. Machol, School of Electrical Engineering, Purdue Univ., Lafayette)

12–14. Agglomeration, intern. symp., Philadelphia, Pa. (Metallurgical Soc. of the AIME, 29 W. 39 St., New York 18)

12–14. Chemical Soc., anniversary meeting, Liverpool, England. (Chemical

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13–14. Society of Technical Writers and Publishers, 8th annual, San Francisco, Calif. (R. B. Meier, Head Editor, Engineering, Stanford Research Inst., 333 Ravenswood Ave., Menlo Park, Calif.)

13–15. Pacific Northwest Metals and Minerals Conf., Spokane, Wash. (Metallurgical Soc. of AIME, 29 W. 39 St., New York 18)

14–16. Association of Clinical Scientists, Philadelphia, Pa. (R. P. MacFate, 323 Northwood Rd., Riverside, Ill.)

17–18. Great Lakes Research, 4th conf., Ann Arbor, Mich. (C. F. Powers, Great Lakes Research Division, 1119 Natural Science Bldg., Ann Arbor)

17–19. Fluid Seal Meeting, intern., Ashford, Kent, England. (Information Officer, British Hydromechanics Research Assoc., South Road, Temple Fields, Harlow, Essex)

17–24. International Congress of Nurses, 12th quadrennial cong., Melbourne, Australia. (Miss D. C. Bridges, Secretary, 1 Dean Trench St., London, S.W.1, England)

18–20. Chemical Reactions in the Lower and Upper Atmosphere, intern. symp., San Francisco, Calif. (R. D. Cadle, Stanford Research Inst., Menlo Park, Calif.)

18–21. American Geophysical Union and American Meteorological Soc., Washington, D.C. (American Geophysical Union, 1515 Massachusetts Ave., NW, Washington 5, D.C.)

19–21. Southwestern Inst. of Radio Engineers Conf. and Electronics Show, Dallas, Tex. (SWIRECO 61, P.O. Box 7443, Dallas 9)

20–21. Society of Chemical Industry, fungicide symp., London, England. (B. J. Heywood, 103 Harrow Drive, Hornchurch, Essex, England)

20–22. Association of Southeastern Biologists, Lexington, Ky. (H. J. Humm, Department of Botany, Duke Univ., Durham, N.C.)

20–24. Microbial Reactions in Marine Environments, intern. symp., Chicago, Ill. (C. H. Oppenheimer, Inst. of Marine Science, Univ. of Texas, Port Arkansas)

21–22. American Assoc. of Univ. Professors, Boston, Mass. (W. P. Fidler, AAUP, 1785 Massachusetts Ave., NW, Washington 6, D.C.)

21–23. American Soc. for the Study of Sterility, annual, Miami Beach, Fla. (H. H. Thomas, 920 S. 19 St., Birmingham 5, Ala.)

23. American Pharmaceutical Assoc., Chicago, Ill. (W. S. Apple, 2215 Constitution Ave., NW, Washington, D.C.)

23–26. American Assoc. of Colleges of Pharmacy, Chicago, Ill. (C. W. Bliven, George Washington Univ., Washington 6, D.C.)

23–27. American Ceramic Soc., 63rd annual, Toronto, Canada. (C. S. Pearce, 4055 N. High St., Columbus 14, Ohio)

23–27. Society of American Bacteriologists, Chicago, Ill. (E. M. Foster, 311 Bacteriology, Univ. of Wisconsin, Madison)

23–28. American Soc. of Hospital Pharmacists, Chicago, Ill. (J. A. Oddis, 2215 Constitution Ave., NW, Washington 7, D.C.)

24–26. Aerospace Medical Assoc., 32nd annual, Chicago, Ill. (W. J. Kennard,

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24–27. American Assoc. of Petroleum Geologists, Denver, Colo. (G. V. Cohee, U.S. Geological Survey, Washington 25, D.C.)

24–27. American Physical Soc., Washington, D.C. (K. K. Darrow, 538 W. 120 St., New York 27)

25–28. Society of Economic Paleontologists and Mineralogists, Denver, Colo. (J. Imbrie, Dept. of Geology, Columbia Univ., New York, N.Y.)

27–28. Diseases in Nature Transmissible to Man, 11th annual southwestern conf., College Station, Tex. (F. P. Jaggi, Jr., Agricultural and Mechanical College of Texas, College Station)

27–28. Health Education Conf., New York Acad. of Medicine, New York, N.Y. (I. Goldston, 2 E. 103 St., New York 29)

27–29. American Acad. of Neurology (members and guests), Detroit, Mich. (Mrs. J. C. McKinley, 4307 E. 50 St., Minneapolis 17, Minn.)

27–29. Wildflower Pilgrimage, 11th annual, Great Smoky Mountains Natl. Park, Tenn. (A. J. Sharp, Dept. of Botany, Univ. of Tennessee, Knoxville)

27–5. American Psychiatric Assoc., annual, Philadelphia, Pa. (D. Blain, 1700 18 St., NW, Washington 6)

28–30. American Psychosomatic Soc., 18th annual, Atlantic City, N.J. (M. F. Reiser, 265 Nassau Road, Roosevelt, N.Y.)

30–4. Aero/Space Instrumentation Symp., 7th annual, Dallas, Tex. (W. J. Gabriel, Route 3, Box 36, Fort Worth, Tex.)

30–4. Electrochemical Soc., Indianapolis, Ind. (R. K. Shannon, 1860 Broadway, New York 23)

30–6. Conference on Internal Medicine, Nassau, Bahamas. (Bahamas Conferences, P.O. Box 1454, Nassau)

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1–3. American Oil Chemists' Soc., St. Louis, Mo. (K. F. Mattil, Swift and Co., U.S. Yards, Chicago 9, Ill.)

2–3. American Pediatric Soc., Atlantic City, N.J. (C. M. Riley, Denver General Hospital, Denver 4, Colo.)

2–3. Association of American Physicians, Atlantic City, N.J. (P. B. Beeson, Yale Univ. School of Medicine, New Haven 11, Conn.)

2–5. Criticality Control in Chemical and Metallurgical Plant, intern. symp., OEEC, Karlsruhe, Germany. (European Nuclear Energy Agency, 38, Boulevard Suchet, Paris 16, France)

2–6. American Assoc. on Mental Deficiency, Cincinnati, Ohio. (N. A. Dayton, Mansfield Training School, Mansfield Depot, Conn.)

3–5. Nuclear Applications in Space Conf., Gatlinburg, Tenn. (J. J. Harford, American Rocket Soc., 500 Fifth Ave., New York, N.Y.)

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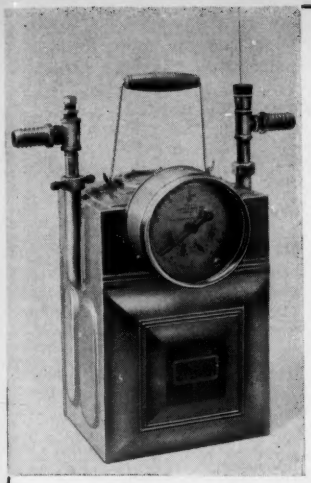

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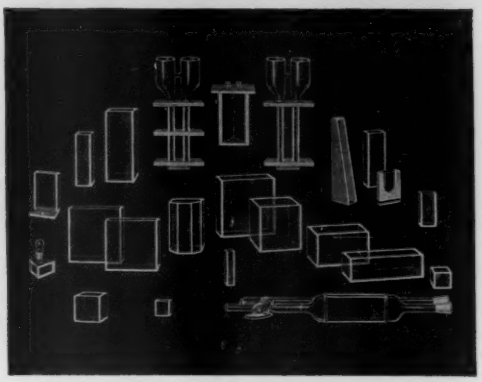

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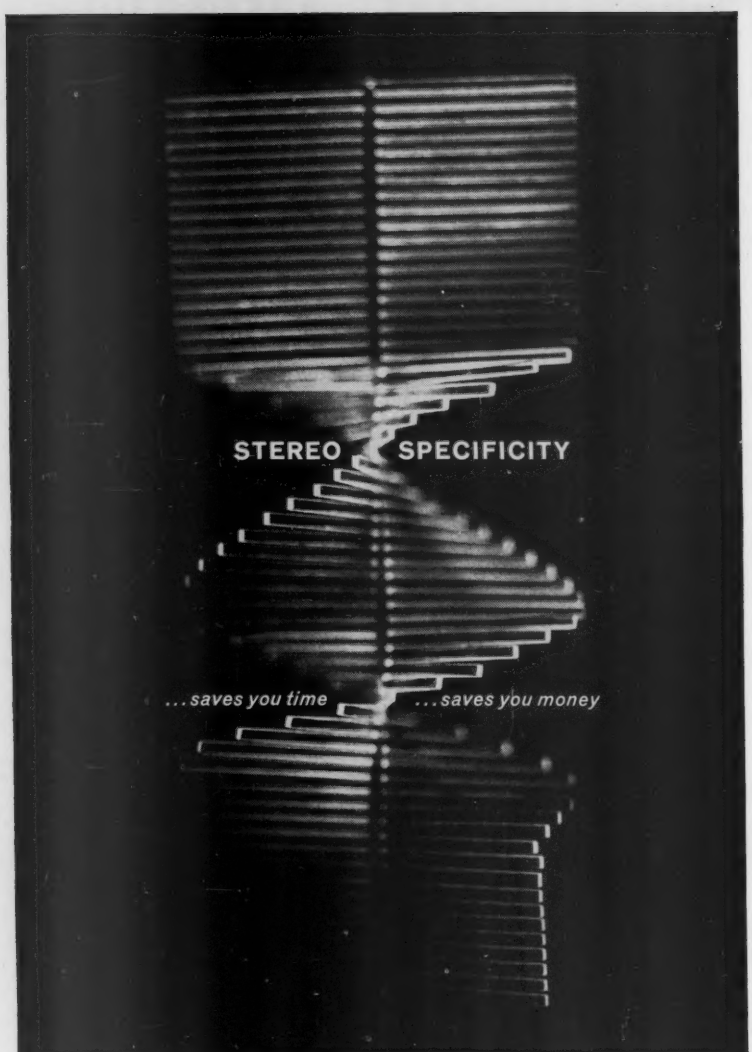

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4–5. Society for Pediatric Research, Atlantic City, N.J. (C. D. West, Children's Hospital, Cincinnati 29, Ohio)

4–6. American Ethnological Soc., Columbus, Ohio. (Miss N. F. S. Woodbury, Arizona State Museum, Univ. of Arizona, Tucson)

4–6. American Philosophical Assoc., western division, St. Louis, Mo. (L. E. Hahn, Washington Univ., St. Louis 30)

4–6. American Soc. of Human Genetics, Atlantic City, N.J. (W. J. Schull, 1133 E. Catherine St., Ann Arbor, Mich.)

4–6. Pediatric Surgery, symp., New York, N.Y. (Office of the Associate Dean, New York Univ. Post-Graduate Medical School, 550 First Ave., New York 16)

4–6. Society for American Archaeology, Columbus, Ohio. (J. B. Wheat, Univ. of Colorado Museum, Boulder)

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5–7. American Soc. of Internal Medicine, Miami Beach, Fla. (G. T. Bates, 350 Post St., San Francisco 8, Calif.)

5–8. American Psychoanalytic Assoc., Chicago, Ill. (Mrs. H. Fischer, 1 E. 57 St., New York 22)

6–7. Academy of Psychoanalysis, annual, Chicago, Ill. (J. H. Merin, 49 E. 78 St., New York 21)

6–9. Circuit Theory, 5th midwestern symp., Urbana, Ill. (M. E. Van Valkenburg, Dept. of Electrical Engineering, Univ. of Illinois, Urbana)

7–10. American Inst. of Chemical Engineers, Cleveland, Ohio. (J. F. Van Antwerpen, AIChE, 25 W. 45 St., New York 36)

7–11. Institute of Food Technologists, New York, N.Y. (C. S. Lawrence, 176 W. Adams St., Chicago 3, Ill.)

7–12. Medical Library Assoc., Inc., Seattle, Wash. (Miss R. J. Mann, Mayo Clinic Library, Rochester, Minn.)

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8–12. American College of Physicians, 42nd annual, Miami Beach, Fla. (ACP, 4200 Pine St., Philadelphia 4, Pa.)

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117th annual, Chicago, Ill. (C. H. H. Branch, 156 Westminster Ave., Salt Lake City, Utah)

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11–13. American Inst. of Industrial Engineers, annual, Detroit, Mich. (W. J. Jaffe, Newark College of Engineering, 367 High St., Newark 2, N.J.)

11–13. American Radium Soc., Colorado Springs, Colo. (C. G. Stetson, 350 Engle St., Englewood, N.J.)

15–16. Co-ordination Compounds, symp., Hamilton, Ontario, Canada. (R. J. Gillespie, McMaster Univ., Hamilton)

15–17. Institute of Radio Engineers, natl. symp., Washington, D.C. (G. Shapiro, National Bureau of Standards, Washington 25)

15–17. Radiation Research Soc., annual, Washington, D.C. (E. L. Powers, Div. of Biological and Medical Research, Argonne National Laboratory, Argonne, Ill.)

15–18. Society of Aeronautical Weight Engineers, Akron, Ohio. (D. B. Block, 4004 Oxford Ave., NW, Masillon, Ohio)

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22–24. American Thoracic Soc., Cincinnati, Ohio. (F. W. Webster, 1790 Broadway, New York 19)

22–24. Global Communications, 5th natl. symp., Chicago, Ill. (R. D. Slayton, 5555 Touhy Ave., Skokie, Ill.)

22–24. Telemetering Conf., natl., Chicago, Ill. (J. Becker, AC Spark Plug Division, General Motors Corp., Milwaukee 1, Wis.)

22–25. American Urological Assoc., Los Angeles, Calif. (W. P. Didusch, 1120 N. Charles St., Baltimore 1, Md.)

22–25. Design Engineering Conf. and Show, Detroit, Mich. (ASME Meetings Dept., 29 W. 39 St., New York 18)

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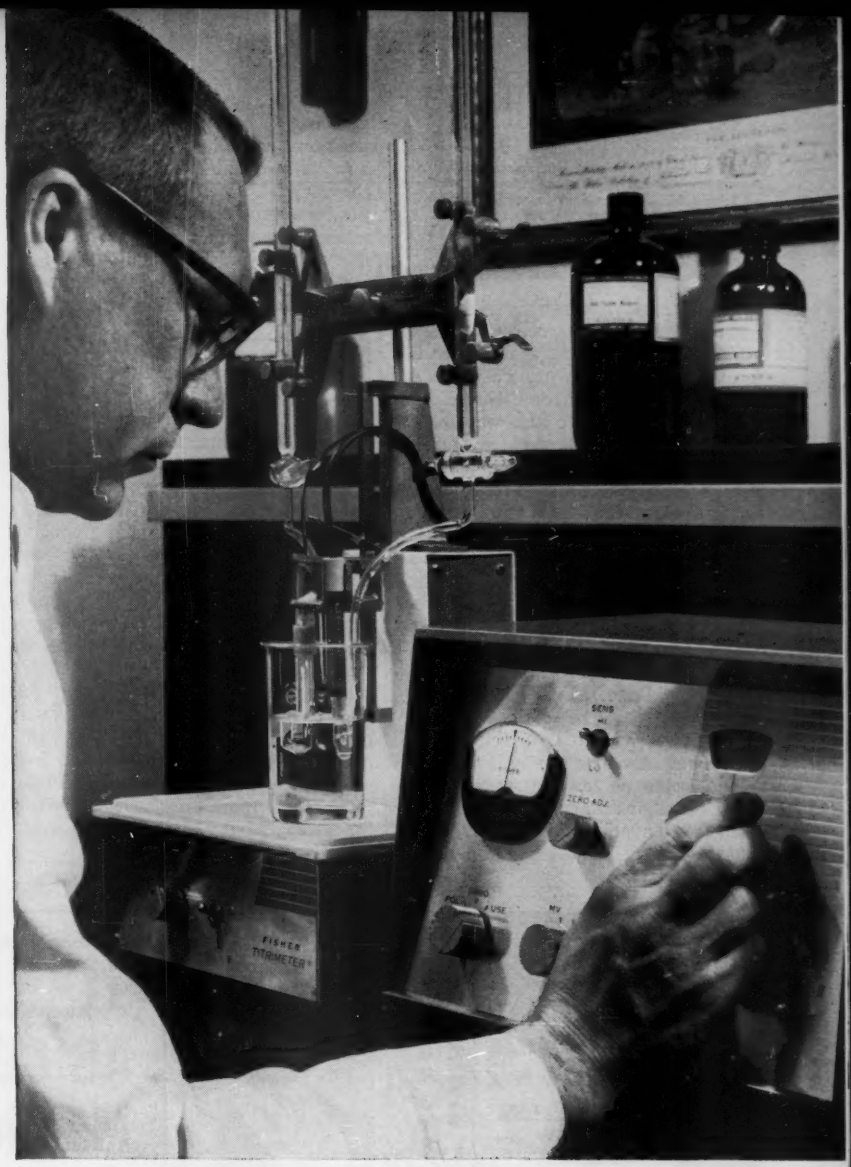

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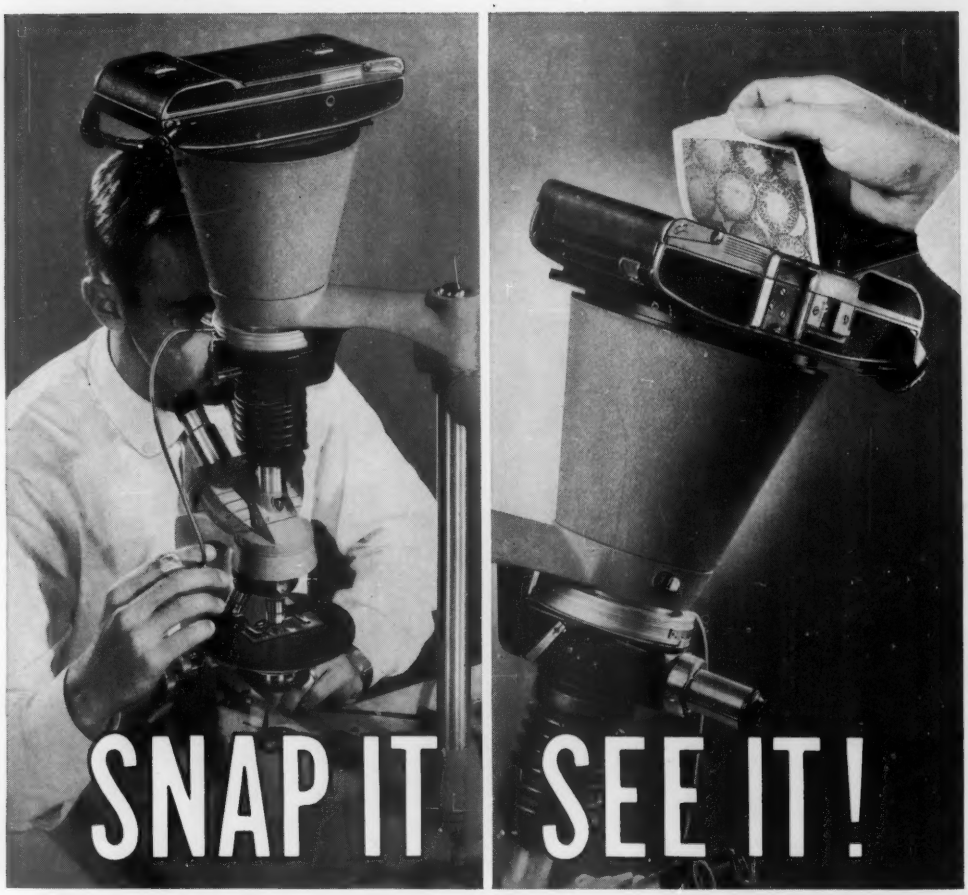

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